Popular Astronomy

Vol. LV, No. 9 NOVEMBER, 1947 Whole No. 549

The Planetary Theory of Ptolemy

By A. PANNEKOEK

I

In Ptolemy's "Mathematical Composition" ancient astronomy found its most perfect and complete exposition. The planetary motions, occupying the second half of this treatise, are explained by means of the theory of the epicycles, the ripest product of ancient science. Its fundamental ideas had been developed by Apollonius and Hipparchus; but it was Ptolemy who worked it out completely.

In representing the motions of the planets the epicycle theory may be said to be entirely equivalent to the later heliocentric theory. If we assume the earth to be at rest and give to each of the celestial bodies its motion in the opposite direction, the sun thus describing a yearly orbit around the earth and the planets getting a second circle besides their own orbits, then the relative motions remain unchanged and the phenomena are exactly the same.

In Greek astronomy all heavenly bodies move in circles. A planet moves on a circle (epicycle) the centre of which describes a greater leading circle (deferent) around the earth as the world centre. For the outer planets (Saturn, Jupiter, Mars) the leading circle is their own orbit, the epicycle is the reflected orbit of the earth. For the inner planets (Venus, Mercury) the leading circle is the reflected earthly orbit, the smaller epicycle is the planet's orbit.

How will the elliptic motion of the planets appear in this world system? The deviation from a circular figure is, except for Mercury, too small to be perceived. But the eccentricity and the variable velocity will show themselves exactly as they would appear if seen from the sun. Since the velocity of the planet is greatest at perihelion, where moreover it is seen at a smaller distance, the effect of the eccentricity on the apparent, *i.e.* the angular, motion is doubled; so it produces an inequality in longitude, an alternative anticipating and lagging relative to a regularly progressing "mean" planet, of double the amount of the real eccentricity. At the same time the epicycle, because, in moving around, it is seen at different distances from the central body, varies in apparent size proportional to the eccentricity itself; the oscillations producing the direct and retrograde motions thus are variable to an amount of the single eccentricity.

Hipparchus had already explained the variable angular velocity of

the sun in its yearly course along the ecliptic by means of the eccentric position of the earth. Since he assumed a constant velocity of the sun the eccentricity had to be double the amount of its real value (0.017); Hipparchus took it to be 1/24, and Ptolemy assumed the same value.

Hipparchus had not worked out a complete epicycle theory of the planets, probably from lack of sufficient observational data. This was left to Ptolemy. His pride in having achieved it is evident in the opening words of the parts devoted to the planets:

"It now being our purpose to prove also for the five wandering stars, just as for the sun and the moon, that their seeming anomalies are all of them fulfilled by means of uniform and circular motions, because these motions conform to the nature of divine beings but disorder and dissimilarity are foreign to them, the happy success of such an enterprise may be considered a great thing, nay in reality the final goal of philosophically directed mathematical science; but for many reasons it is difficult and therefore naturally has not been undertaken successfully by anyone as yet." (Book IX, Chapter 2).

Ptolemy was an active observer; clearly he made many more observations than the small number selected among them which he quotes and uses in his book. It is said that his observations are less precise than those of Hipparchus; the positions of the stars are given by him, according to Dreyer, to ¼°, by Hipparchus to ⅙°. The errors of the planets' positions, however, are much larger. Ptolemy says that he used for his demonstrations only such observations as offered the best warrant of reliability: either conjunctions with, or near approaches to, fixed stars or the moon; or measurements with the astrolabe. This instrument, composed of graduated rings corresponding to the celestial circles, was first turned into the true position by means of a star, or the sun or the moon; then the planet was observed through diopters and the circles were read. Besides his own observations between 130 and 142 A.D. he made, for earlier years, use of observations by Theon at Alexandria. The periods of revolution which he took from Hipparchus, probably came from the Chaldeans; in some cases he used slightly corrected values.

II

Theory is most simple and in conformity to our ideas for the outer planets. The planet travels along the epicycle with constant velocity in such a way that it keeps in pace with the sun in its orbit, *i.e.* that the radius from the epicycle-centre C to the planet P remains always parallel to the radius from the centre of the sun's orbit to the sun. The arc it performs is counted from the outermost point of the epicycle F, farthest removed from the centre of the deferent; the ancients considered that outer point as being at rest when the epicycle was carried around, just as if, like a wheel, it were fixed at the end of a turning rod. That arc is called "anomaly relative to the sun"; Figure 1, where it

is represented by FP, shows that it is equal to the difference between the longitude of the sun (direction ES or CP) and the longitude of the epicycle's centre (direction EC), called "mean longitude of the planet." When the arc completes 360° the planet returns to the outer point of the epicycle, it stands again in conjunction with the sun, and has performed a (synodic) planetary period. The deferent is an eccentric circle, with the earth E somewhat outside the centre M; it is usually called the "excenter" by Ptolemy. The arc VC on this excenter performed by the epicycle's centre, counted from the vernal point V, is the "longitude relative to the ecliptic"; if it reaches 360° a revolution of the planet is completed. Such is the original form of the epicycle theory. But Ptolemy perceived that it was not sufficient.

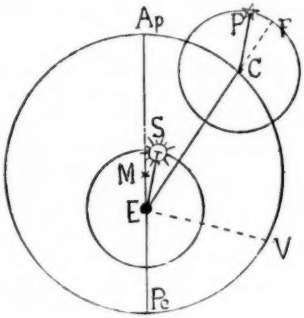

FIGURE 1

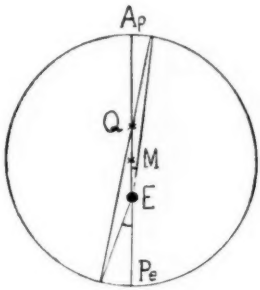

FIGURE 2

"Now we found, however," he says, "by our continued testing and comparison of the successively observed courses with what by reasoning resulted from the combination of the two hypotheses, that the course of motion cannot be so simple." First the directions of apogee and perigee (points of the deferent most and least remote from the earth, *Ap* and *Pe* in Figure 2) partake in the precession of the stars. "We found that first the apogees of the excenters make a small progress in the direction of the signs, uniform again around the centre of the ecliptic and for all the planets nearly the same as has been observed for the sphere of the fixed stars (*i.e.* one degree in one hundred years), in so far at least as it is possible to form an idea from the available data. Secondly we found that the centres of the epicycles go round on circles of the same size as the excenters causing the anomaly, but having another centre: for the other planets the centres of these circles are situated midway between the centres of the above-named excenters and the centre of the ecliptic." (Book IX, Chapter 5). Expressed in other words: the point around which the epicycle's centre revolves with constant angular velocity—afterwards called *punctum aequans*, equality-point, equant, *Q*

in Figure 2, is not the centre of the circle described but is situated as far outside this centre M to one side as the earth (the "centre of the ecliptic") is distant toward the opposite side.

This is indeed an excellent representation of what at the time could be observed of the planetary motions. The distance of the epicycle's centre to the earth, hence the apparent size of the epicycle, varies according to the eccentricity ME. That the arcs described in equal times appear as equal angles as seen from the equant means that in reality the velocity at apogee is smaller, at perigee is larger; seen from the earth at the other side of the centre the difference in angular velocity shows the double effect of the eccentricity. Thus the distance earth-to-equant, often called the total eccentricity, determines the variation in angular velocity, whereas the distance earth-to-centre of the circle is what we call the real eccentricity. In the motion of the sun only the former presents itself.

Ptolemy does not give any observations to demonstrate this theory. He could have done so easily; observations of Mars far away from opposition, once in the vicinity of apogee, another time in the vicinity of perigee, would clearly show the differences in apparent size of the epicycle. Nothing of the sort is found; he simply states the theory. Of course he must have found it from observations. In his introduction he says:

"When we are compelled to premise certain axioms not deduced from a visible datum but found by coherent trying and inferring . . . we do it with the knowledge . . . that axioms given without demonstration, if they prove to be concordant with observation, cannot have been found without methodical argument though it is difficult to explain how they have been derived . . ." (Book IX, Chapter 2). We may be sure that observations have shown to him the working eccentricity in one case nearly half as large as in the other case, and that the assumption of an exact ratio 1:2 was a theoretical but lucky hit. He presents the result in this way: "Also for these planets the eccentricity, deduced from the greatest deviation in the anomaly relative to the ecliptic, was found to be the double amount of the eccentricity derived from the retrograde motion in the cases of the largest and smallest distance of the epicycle." (Book X, Chapter 6).

Ptolemy's first task is now to ascertain, for the planets Mars, Jupiter, Saturn, the elements of their excenter, *i.e.*, the eccentricity and the direction of the line of apsides, expressed by the longitude of the apogee. In this he makes use of three oppositions, the moments when the planet is at the deepest point of the epicycle, nearest to the earth, because at such a time the observed longitude of the planet coincides with the longitude of the epicycle's centre. We call it opposition, but it is not opposition to the real sun but to the "mean" sun; it is not the line earth-sun but the line centre of solar circle-sun that turns regular-

ly in pace with the planet on its epicycle; in this Figure 1 was not entirely exact. By comparing for every day the longitude of the mean sun, taken from his tables, with the observed longitude of the then retrograding planet, the moment and the longitude of opposition could easily be derived. "Thus first we ascertained for Mars three oppositions. We observed it in Hadrian year 15, at 26/27 of the month Tybi (transformed into our calendar December 15/16 of 130 A.D.) 1^h in the night at Gemini 21° (*i.e.*, longitude 81°); in Hadrian year 19, at 6/7 Pharmuthi (February 21/22 of 135 A.D.) 9^h in the evening at Leo 28° 50′, (*i.e.*, longitude 148° 50′); in Antonin year 2 at 12/13 Epiphi (May 27/28 of 139 A.D.) 2 hours before midnight at Sagittarius 2° 34′ (i.e., longitude 242° 34′)." (Book X, Chapter 7.)

Now the problem is a purely geometrical one. Asked is the position of the earth E within the eccentric circle. Given are the longitudes of the planet at I, II, III (Figure 3), hence the angles 67° 50′ and 93° 44′

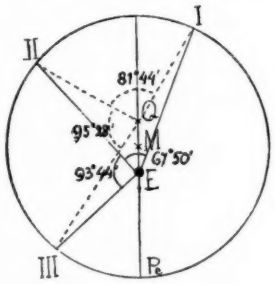

FIGURE 3

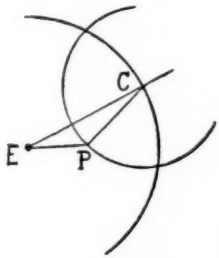

FIGURE 4

at E. Moreover the time intervals, diminished by a certain number of full revolutions, determine the proportional angles at the *punctum aequans* Q: 81° 44′ and 95° 28′. Ptolemy remarks that this problem is too difficult to be solved in a direct way. Hence he solves it by a series of approximations. He first supposes Q to be the centre of the circle, and finds distance and direction of EQ. Then he computes what difference in the three observed longitudes the transition to a circle with centre midway between E and Q would produce; after a new computation with the corrected values the process is repeated once more. In this simplified form Ptolemy's problem was identical with what in modern geodetics is called Pothenot's or Snell's problem: to determine a position between three well-known points by measuring at the unknown station the directions to these three points. His solution is straight-forward, of course somewhat more clumsy than it is with our modern trigonometrical formulas, because he had at his disposal only his table of chords for every angle (the ancient form for what we

know as a table of sines) and Pythagoras' theorem, with now and then an appeal to some proposition of Euclid. The results were:

First computation $EQ = 0.219$, arc III $- Pe = 39° 19'$.
Corrections to I, II, III $-0° 32'$, $+0° 33'$, $-0° 50'$.
Second computation $EQ = 0.197$, arc III $- Pe = 45° 33'$.
Correction to I, II, III $-0° 28'$, $+0° 28'$, $-0° 40'$.
Third computation $EQ = 0.200$, arc III $- Pe = 44° 21'$.

From the latter value the angle III EPe is computed to be $52° 56'$, so the apogee is situated at Cancer $25° 30'$, *i.e.*, at longitude $115° 30'$.

His second task was to derive the size of the epicycle relative to the excenter. It can be done most easily by using an observation of the planet outside its opposition, when it stands far sideways on its epicycle. For this derivation Ptolemy, most curiously, gives an observation only 3 days after opposition. "Since it is our further aim to fix numerically the relative size of the epicycle, we chose an observation we made nearly 3 days after the third opposition, in Antonin year 2 at 15/16 Epiphi (May 30/31 of 139 A.D.) 3^h before midnight." With the astrolabe directed by means of Spica (longitude $176° 40'$) the longitude of Mars was found to be $241° 36'$. Mars at the same time was found to stand $1° 36'$ east of the moon (which according to the tables stood at $239° 20'$, corrected $40'$ for parallax giving $240° 0'$). Hence also in this way of reckoning "a position of Mars was found $241° 36'$, agreeing with the other result." (Book X, Chapter 8.) The exact concordance of the minutes of arc clearly indicates that they are fitted on purpose. If we try to verify the computation we find the longitude of the moon from the tables $13'$ larger; since, however, the time is only given in hours and the moon moves $33'$ per hour, every value within this range might be taken; the choice has been made in such a way that the concordance was absolute.

With these data it is again simply a computation by geometry. The method followed comes down to this: Mars as seen from the earth had retrograded $0° 58'$ since the opposition; the centre of the epicycle was computed to have proceeded $1° 45'$, so that relative to this centre the planet has regressed $2° 43'$, whereas on the epicycle an arc of $1° 8'$ had been described. From the triangle EPC (Figure 4) with $\angle E = 2° 43'$ and $\angle C = 1° 8'$ and the distance EC at the time 0.934 we find CP, the radius of the epicycle to be 0.658. The true value is 0.656. It is clear that this close accordance is either chance luck or design. If errors of $15'$ are allowed in both observations, at opposition and 3 days afterwards (the real errors may be considerably larger) then the result may be 0.030 different. So we may take it for certain that his value for the size of the epicycle does not rest simply on this observation, but has been derived from other and more favorable observations with angles of $20°$ instead of $2°$. Then the derivation given in his book has to be considered as a demonstration of the geometric method followed; in

that case it may be understood that the figures of the minutes are adapted and chosen in such a way as to give the good result. It must be added that computed with modern data the longitude of Mars at the moment of his observation was 242° 16′, hence 0° 41′ larger. It is well known that owing to an error of his vernal point of about 1° all longitudes of Ptolemy, including those of the solar tables, are too small; for Spica the error in his catalogue was 1° 19′, so that in the measurement of Mars an error of 39′ was made.

In the same way as is explained here for Mars Ptolemy derived the elements for Jupiter and Saturn. His results are contained in the following small table, where for the sake of comparison the true values according to modern data are added.

TABLE 1

ELEMENTS OF THE OUTER PLANETS

	Mars		Jupiter		Saturn	
	Ptolemy	True	Ptolemy	True	Ptolemy	True
Total eccentricity	0.200	0.186	0.092	0.096	0.114	0.112
Long. of apogee	115°5	121°	161°	164°	233°	239°
Radius of epicycle	0.658	0.656	0.192	0.192	0.108	0.105

The smallness of the differences shows in how excellent a way ancient astronomy, in Ptolemy's work, succeeded in representing the phenomena of these planets.

III

For the inner planets the problem is more difficult, because the premises of the theory fit less well the phenomena. Here the epicycle is our real orbit of the planet and is supposed to be a circle described with constant velocity. The deferent represents our orbit of the earth, hence should be identical with Ptolemy's solar orbit. His theory, however, only demands that the radius in both have always the same direction, *i.e.*, that the mean longitude of the planet is equal to the longitude of the mean sun. The real sun is entirely disregarded; the identity of the mean motions secures that always Mercury and Venus go on oscillating from one side of the sun to the other. Ptolemy in his theory deals only, as data of observation, with the greatest elongations east and west; he has to represent them by the motion solely of the epicycle's centre along the deferent.

For Venus matters are facilitated by its small eccentricity, in modern theory 0.007 only with perihelion at 105°; the greatest elongations vary between 45° 53′ and 46° 43′ only. This is relative to the true sun which on account of the eccentricity of the earth's orbit (0.017 with perihelion at 70°) at longitude 340° is 1° 57′ ahead, at longitude 160° is 1° 57′ back relative to the mean sun. At a solar longitude of 15° (and 195°) the orbit of Venus is seen sideways, with the centre 0° 22′ behind (or before) the sun. Thus Venus' advancing and retarding relative to the mean sun (*i.e.*, to mean Venus as well) is a combination of both, chiefly

determined by the solar eccentricity, slightly modified by that of Venus, amounting to 1° 40′ ahead at longitude 333°. In such a way, with eccentricity 0.029 and apogee at 63° the phenomena can be rendered by such a theory as Ptolemy's.

Ptolemy makes use of 8 greatest elongations of Venus observed by himself (5) or by Theon (3); we have collected his data in Table 2. He first looks for the direction of apogee and perigee, for which he takes two greatest elongations of the same amount, to the western and the eastern side; since they can only be equal at equal distance at both sides of the line of apsides, this line must fall midway between the observed longitudes. This is the case with the first pair; its mid-value is 55°; it is also the case with the second pair, with mid-value 235°. Which of them is the longitude of apogee? The third pair gives the answer; with the mean sun near 55° the greatest elongation is smaller, near 235° it is larger; hence the former is the apogee. The fourth pair, with the mean sun at a 90° different longitude, shows the maximum amount of the inequality of the greatest elongations; half the difference, 2° 22½′ corresponds to an eccentricity of the deferent of 0.041 or 1/24.

TABLE 2

GREATEST ELONGATIONS OF VENUS

	Data of Ptolemy					Computed Values		
Date of greatest elong.	Time	Long. ♀	Mean long. ☉	Elongation rel. to mean ☉	Long. ♀	Elongation rel. to mean ☉	Date of greatest elong.
132 March 7	7^h ev.	31° 30′	344° 15′	+47° 15′	31° 13′	+46° 57′	Feb. 21
140 July 30	4½^h mo.	78 30	125 45	—47 15	80 29	—46 9	July 7
127 Oct. 11	6½^h mo.	150 20	197 52	—47 32	150 35	—47 14	Sept. 23
136 Dec. 25	6½^h ev.	319 36	272 4	+47 32	319 57	+46 53	Dec. 4
129 May 20	5^h mo.	10 36	55 24	—44 48	11 58	—44 23	Apr. 30
136 Nov. 18	5^h ev.	282 50	235 30	+47 20	282 4	+45 32	Dec. 4
134 Feb. 18	6^h mo.	281 55	325 30	—43 35	281 58	—44 31	Feb. 17
140 Feb. 18	5½^h ev.	13 50	325 30	+48 20	14 30	+47 59	Feb. 18

What could be expected, *viz.*, that the deferent of Venus shows, somewhat modified, the eccentricity of the earth's orbit, comes out indeed; however, not our true but Ptolemy's too large value, because it entered by means of his solar tables. The curious fact is that Ptolemy himself seems not to have perceived, at least does not mention, the exact coincidence of this result 1/24 with the eccentricity of the sun's orbit, or the near coincidence of the apogee. To him the orbit of the sun and the deferent of Venus clearly are entirely unrelated things that only have the same mean longitude and mean motion.

The third pair of elongations, at apogee and perigee, allows the derivation of some numerical values. The radius of the epicycle, in unit distance of its centre to the earth, is given by the sine of the greatest elongation; for apogee and perigee we find sin 44° 48′ = 0.7046 and sin 47° 20′ = 0.7353. Their mean 0.720 is the semi-diameter of the epicycle; half the difference, percentual, 0.0153 : 0.720 = 1/47 is the

eccentricity of the observing point, the earth. It is just half the amount of the total eccentricity found above; hence the Venus-deferent has an equant. Since Ptolemy, obviously, was not aware of the identity of this deferent with the sun's circle, Kepler had afterwards to discover the existence of this equant as a new fact. So it is not exactly true what we said above that he nowhere gives a demonstration of the bisection of the eccentricity; here for Venus he derives the total as well as the simple eccentricity. The exactness of the ratio 1:2 again is suspicious; a change of only 15′ in one of the greatest elongations, smaller than errors which are to be expected, would change the result one-fifth of its value.

There is more in the figures to be wondered at. How did he manage to have the pairs of greatest elongation so exactly equal even to the single minutes, whereas his instruments could only be read to ¼ or ⅙ of a degree? And the results of estimates from conjunctions and near appulses often were more erroneous. By computing the elongations according to modern data—using P. V. Neugebauer's "Tafeln zur astronomischen Chronologie"—we found the values in the right hand columns of Table 2, showing errors in Ptolemy's data of sometimes more than a degree. And how did he manage to have greatest elongations just at moments when the sun's mean longitude had the desired value of 55° or 235°? These eight phenomena of greatest elongation in reality took place at fixed, determinate days not to be influenced by his needs, and he had nothing out of which to choose. If, then, we compute the real dates of greatest elongation (right hand part of Table 2), it appears that his dates deviate, sometimes up to 20 days. It is, indeed, hardly possible to determine the exact moment of observation, as the change is then nearly imperceptible; 20 days before and after the elongation has decreased only 1° 9′ and 1° 54′. So, if Ptolemy says: "We observed Venus on Dec. 25, 136 A.D., in greatest elongation from the sun," he understands under that name a longer period, more than a month, during which no change of elongation is perceptible. This is shown quite clearly where, shortly after having used this elongation of Dec. 25 he says: "We observed 136 A.D. on Nov. 18 Venus in greatest elongation." This represents the same greatest elongation, which in reality took place on Dec. 4. Out of such an extended period of greatest elongation he took such observations as satisfied his needs as to the longitude in the orbit; if they did not exactly fit he could interpolate the result for predeterminate conditions. Here again we get the impression that for working out his theory more observations have been used, and that in his book he has chosen such as were required for an orderly geometric demonstration.

IV

Mercury presented far greater difficulties; indeed its large eccentricity (0.21 with perihelion at 48°) makes its motion highly unfit to be

represented by the epicycle theory. The greatest elongation from the true sun varies between 27°.8 and 17°.9; the centre of its ellipse that has to follow the deferent oscillates to both sides of the sun to an amount of 4°.7 sin (L — 48°) (for L = 138° when the ellipse is seen sideways the centre is $0.21 \times 0.387 = 0.0812 = 4°.7$ ahead of the sun). The true sun deviates, as we have seen, an amount of 2°.0 sin (L —250°) from the mean sun; adding the two oscillations we find that in total the centre of Mercury's epicycle advances or follows its mean position by 2°.8 sin (L —32°). An excenter with total eccentricity 0.049 and apogee at 212° can represent it.

The apparent size of the epicycle, however, visible in the sum total of east and west elongations, in this case does not vary only by virtue of the eccentric orbit of the earth which makes it deviate from its mean value 22° 50′ by 1/60 in surplus at 70°, by 1/60 smaller at 250°, but also by the elliptic shape of Mercury's orbit. When looking in the direction of the line of apsides (at 48° and 228°) we see the transverse axis to be 1/45 smaller than the great axis. Combining these changes we find that the greatest elongation at longitudes of about 50° to 70° is $1/60 + 1/45 = 1/26$ smaller (nearly 1°), at 140° — 160° and 320° — 340°, nearly normal; and at about 230° — 250°, $1/45 - 1/60 = 1/180$ smaller, hence practically normal also. To represent these variations the deferent should exhibit in three quadrants nearly the same distance to the earth, but in the fourth quadrant (about 50° — 70°) a distance 1/26 larger. This is not only inconsistent with the excenter just derived but cannot even be represented by a circle, being rather egg-shaped.

Now in Ptolemy's theory we find just such an orbit, produced by an effectual geometric construction. The centre of Mercury's deferent is not a fixed point but describes yearly in the opposite direction a small circle with radius 0.05 and centre at a distance 0.10 from the earth. In drawing the figure we see that the orbit now is changed into an oval extended in the line of apsides, flattened in transverse direction. The distance to the earth in perigee is 0.95, in apogee 1.15, but at 60° to both sides of perigee it is smallest, 0.93; hence only in one quadrant it exceeds perceptibly the value in other directions. At first sight this seems to be a remarkable concordance; but it disappears when we see that the apogee is situated at 190°, not far from that of our first-mentioned excenter, and the excess in distance is 1/5 instead of 1/26. So his complicated non-circular orbit cannot have any reality.

He derived it, at least demonstrates it, in the same way as for Venus. He makes use of 4 pairs of observed greatest elongations, which are collected in our Table 3. The first two pairs fix the line of apsides in the midst of the two solar longitudes, at 10° and 190°; the third pair decides that 190° is the apogee. Moreover this pair of elongations shows the distances in apogee and perigee to have the ratio 23:19, conforming

TABLE 3

GREATEST ELONGATIONS OF MERCURY

Date of greatest elong.	Time	Data of Ptolemy: Long. ☿	Mean long. ☉	Elongation rel. to mean ☉	Computed Values: Long. ☿	Elongation rel. to mean ☉	Date of greatest elong.
132 Feb. 2	Ev.	331°	309° 45′	+21° 15′	330° 29′	+19° 46′	Jan. 29
134 June 4	Mo.	48 45	70 0	−21 15	51 9	−19 46	June 10
138 June 4	Ev.	97	70 30	+26 30	97 7	+25 34	June 10
141 Feb. 2	Mo.	283 30	310 0	−26 30	285 44	−25 17	Feb. 2
134 Oct. 3	Mo.	170 12	189 15	−19 3	170 33	−19 39	Sept. 27
135 April 5	Ev.	34 20	11 5	+23 15	34 18	+22 10	April 5
130 July 4	Ev.	126 20	100 5	+26 15	127 14	+26 10	July 6
139 July 8	Mo.	80 5	100 20	−20 15	83 53	−20 20	July 6

to theory (1.15 and 0.95), and the radius of the epicyle to be 0.343. Ptolemy points out that by adding the east and the west greatest elongations in the 1st and 2nd pair, at 70° and 310° longitude, we get 47° 45′, a little bit larger than $2 \times 23°\ 15'$ in perigee, confirming his theory that there the distance is least. The 4th pair, with the mean sun at 90° from perigee, was used to demonstrate the geometric details of his model. At that time, with the line from the equant Q (Figure 6) to the epicycle's centre C perpendicular

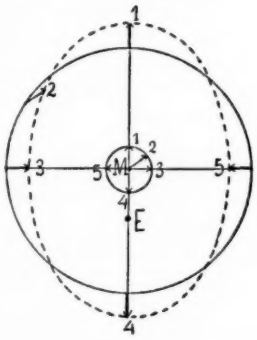

FIGURE 5

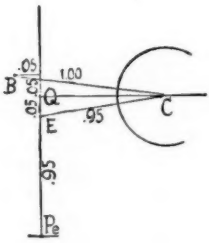

FIGURE 6

to the line of apsides, the epicycle is seen from the earth E under an angle $26°\ 15' + 20°\ 15' = 2 \times 23°\ 15'$, just as large as in perigee; hence the distance EC is equal to EPe, *i.e.*, 0.95. Half the difference $\frac{1}{2}(26°\ 15' - 20°\ 15') = 3°$ determines the distance $QE = 0.05$. Comparing the different lines in the figure we see that indeed the momentary center B is at a distance 1.0 from C, in agreement with the proposed model.

But this model, however ingeniously constructed, does not agree, as we saw, with reality which does not allow this strongly elongated form. The reason for the discrepancy must be sought in the difficulty and the scarcity of the observations. Mercury can be seen only during short

periods in the twilight near the horizon; then it was observed with the astrolabe directed before by means of one of the bright stars, Aldebaran, Regulus, or Antares. A computation of the real longitudes of Mercury from modern data, given in the right hand columns of Table 3, shows, even taking account of all longitudes of Ptolemy being nearly 1° too small, errors up to some degrees; and such errors may spoil all conclusions from equal elongations. Moreover the time of observation often strongly deviates from the true day of greatest elongation, and then the assumption for the case of equal distance to perigee or apogee does not hold. In the 8th elongation Ptolemy has, as Manitius remarked, made an error of 3° in the computation from his own solar table, but it just compensated his error of measurement. So the shortcomings of his theory are easily explained. When, however, we consider that Mercury up to the 17th century, and still later, balked the ingenuity of theorists in representing exactly its motion, we cannot but admire that Ptolemy succeeded in rendering at least the chief features, by means of an inequality of nearly 3° having its maximum at longitudes 100° and 280°. But in the minor details of his artfully designed geometric model his theory was insufficient.

Ptolemy then applies an analogous treatment to a number of old observations of elongations of the years 262-236 B.C., taken from Hipparchus. He derived therefrom that the apogee then had a longitude of 186°, 4° smaller than in his time, so that the apogee took part in the precession of the stars. Considering that a small error in the longitudes produces a twenty times larger error in the apogee, it is evident that this result is entirely fictitious.

V

After the longitudes have been finished the latitudes of the planets must be explained by means of inclinations of the orbits. For the outer planets the excenter, their own orbit, is inclined to the ecliptic, but the epicycle should be parallel to the ecliptic, hence in his model should show the same inclination to the excenter and the same line of nodes. Ptolemy makes a more general assumption: the line of nodes is the same but the angles of inclination are different, to be determined from observation.

For Mars, where the line of nodes is nearly perpendicular to the line of apsides, he observed the latitude at opposition in perigee to be 7°, in apogee 4⅓°. The distances in these points were $0.90 - 0.66 = 0.24$ and $1.10 - 0.66 = 0.44$ times the radius of Mars' orbit. Calling the two inclinations i_1 and i_2, we will have according to Figure 7 $i_1 + (0.66/0.24)\, i_2 = 7°$ and $i_1 + (0.06/0.44)\, i_2 = 4\frac{1}{3}°$; hence $i_1 = 1°$, $i_2 = 2°\ 15'$. Are they really different? If the observed latitudes had been slightly different, 6⅔° and 4½° (the true values are 6° 43′ and 4° 37′), both inclinations would have been found nearly equal, about 1°.8 (the true value is 1° 51′).

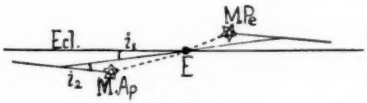

FIGURE 7

For Jupiter and Saturn "we did not find a difference in latitude in the oppositions in perigee and apogee." (Book XIII, Chapter 3). Hence he compares the latitude in conjunction and in opposition. The data he makes use of look very coarse: for Jupiter 1° and about 2°, for Saturn 2° and about 3°. From the distance ratios then he finds: for Jupiter $i_1 = 1\frac{1}{2}°$, $i_2 = 2\frac{1}{2}°$ (true $i = 1° 19'$), and for Saturn $2\frac{1}{2}°$ and $4\frac{1}{2}°$ (true $i = 2° 30'$). The greatest northern deviation he takes to occur at longitudes +20° and —50° different from apogee, hence the ascending nodes for Mars, Jupiter, Saturn are 25°, 91°, 93° (true values 35°, 81°, 98°).

For Venus and Mercury matters should be easy because here only the epicycle has to be inclined to the deferent which coincides with the ecliptic. But the complicated structure of his model is made still more difficult by the introduction of his inclination-mechanism. As a main feature he has, of course, an inclined epicycle, for Venus $2\frac{1}{2}°$ with the ascending node at 55° coinciding with apogee (real values 3° 24′ and 59°), for Mercury $6\frac{1}{4}°$ with the node at 10° coinciding with perigee (real values 7° 0′ and 26°). But he adds an oscillation of the deferent, for Venus between 0° and $+\frac{1}{6}°$, so that the centre of the epicycle remains always at the northern side, for Mercury between 0° and $-\frac{3}{4}°$, so that here it is always at the southern side. This superfluous complication, of course, is too small to be secured by the rather rough observations of latitude.

Ptolemy points out that such a model as he devised for the planetary orbits requires an additional mechanism. In the epicycle theory the epicycle is assumed to be fixed, as it were, on the deferent, so that in revolving along the deferent the outer point remains at the outside; if in an inclined epicycle this point is highest above the plane of the deferent it remains highest while revolving, so that the line of nodes continually turns around. In order to keep the epicyle always in the same situation in space, Ptolemy introduces a special contrivance; the inner-point of the epicycle is situated upon a small vertical circle having its centre in the ecliptic, so that in the period of revolution this inner point is oscillating up and down between the extreme northern and southern deviation. It may look somewhat artificial and mechanical, but it works. To make it still more perfect it is made as eccentric as the deferent itself, so that even the small variations of velocity in longitude and latitude keep pace. And then Ptolemy concludes with the following philosophical reflection.

"And let nobody, looking at the imperfection of our human contrivances, regard the hypotheses here proposed as too artificial. We may not compare human things with things divine nor, in order to demonstrate such enormous things, make use of entirely incomparable examples. For what indeed is more dissimilar than beings always behaving in the same way and creatures never doing so? What more dissimilar than creatures who may be disturbed by any trifle and beings that will never be disturbed, not even by themselves? Of course we must try as far as possible to adapt the simpler hypotheses to the heavenly motions; if, however, we cannot succeed in this way, we must go to hypotheses allowing of such an adaptation. For if once all celestial phenomena can be explained on the basis of the hypotheses, why then should anybody wonder at the possibility that such a complicated interlocking should be inherent to the motions of the heavenly bodies, since they are not subject to any constraint of nature. . . The simplicity itself of the celestial processes may not be judged according to what is held simple among man, the more so since on earth there is no unanimity about what is simple. For if we should look at it from this human point of view, nothing of all that happens in the celestial realms would appear to us to be simple at all, not even the very immutability of the first (daily) rotation of heaven, because for us human beings this very unchangeableness, eternal as it is, is not only difficult, but entirely impossible. But in our judgment we have to proceed from the immutability of the beings revolving in heaven itself and of their motions; for from this point of view they would all appear to be simple, and even simple in a higher degree than what on earth is regarded as such, because no trouble and no pains can be imagined with regard to their wanderings." (Book XIII, Chapter 2).

VI

With the numerical derivation of the elements of the planetary orbits the task was not finished. They had to be adapted to practical use, by means of tables. In the 9th book tables are given, and their practical use is explained, for the mean motions of the planets in longitude and in anomaly, *i.e.*, along the epicycle; in the 12th book the first and the second inequality (for eccentricity, and for epicycle) are given; and the 13th book contains tables for the latitudes. Moreover tables and directions for computation are given for remarkable phenomena, for the turning points between direct and retrograde motion, as well as, in the case of Mercury and Venus, for the greatest elongations. Finally tables are added to derive the heliacal risings and settings of the planets, which played an important role in the first observations of ancient astronomy. Thus the foundation was laid upon which in four following books—afterwards usually separately published under the name "Tetrabiblon"—the theory of astrology, the doctrine of the relation between heavenly and earthly phenomena could be erected.

VII

In this summary of Ptolemy's exposure of the planetary motions we ever and again met with modes of treatment that looked uncommon or even suspicious, at variance with our ideas on scientific work. Data exactly equal that never could have been given thus by his rather coarse observations, as well as good results derived by good geometrical computation from fictitious differences in observed quantities, have sometimes raised the suspicion, or even the accusation that he had simply invented his observations, or at least had forged or doctored them in order to make them harmonize with his theory. So we read in Delambre's great work "Histoire de l'astronomie ancienne, 1817," in the "Discours Préliminaire."

"Has Ptolemy himself observed? The observations which he tells us he has made, are they not computations from his Tables, and examples for the use of better understanding of his theories?" (p. xxxv). "As to the chief question, we see no means to decide it. It seems hard to deny absolutely that Ptolemy has made observations himself, at least if he did not find elsewhere what he wanted for his researches. If he had in his hands observations in greater number, as he says himself, we may reproach him that he did not communicate them and that nowhere he tells what might be the possible errors of his solar, lunar, and planetary tables. An astronomer who today should act in this way would be certain to inspire no confidence at all. But he was alone, he had no judges and no rivals; a long time he has been admired on his own word; and at present it is not deemed worth while to compute the few observations he has left. . ." (p. xxxiv-xxxv).

Delambre here explicitly proclaims the methods of his time to be the standards for judgment of scientific work for all centuries. It is clear, however, that we cannot judge Ptolemy's work according to the ideas and habits of modern scientific research, and that we come to a false and unjust appreciation if we do. We have to place ourselves entirely in the ways of thinking of antiquity, and to realize their differences from our scientific practice. This holds in the first place for the relation of experience, observation to theory.

There is no better way to perceive this difference than another work of antiquity, the small writing of Aristarchus "On the Sizes and the Distances of Sun and Moon," the only one of his works that still exists. From the observation that the moon is exactly divided in two equal halves when its angular distance from the sun is 3° less than a right angle, he deduces that the sun is 19 times more distant than the moon. We should wish to know by what observations he arrived at this amount of 3°, on which the result depends; any pupil of a secondary school then immediately infers the ratio 19. In Aristarchus' book we find at the start six "hypotheses," each some few lines only, of which the third reads: "When the moon appears to us halved its distance from the sun

is less than a quadrant by one-thirtieth of a quadrant." That is all. Thereupon 14 pages follow with geometrical demonstrations to derive that in this case the sun is less than 20 times and more than 18 times as far remote as the moon.

The theoretical apparatus in scientific research, the mathematical methods nowadays at our disposal, have acquired so high a degree of perfection, we have imbibed them at school already so thoroughly that the working up of the data of experience offers no difficulty at all. Hence all attention of modern astronomers is directed to observation, to its errors and uncertainties. Their communication of results bears the character of a protocol, of precise authentic statement of facts, with circumstances and details, which, once given cannot be changed any more. Research of nature is a socially organized and standardized world-wide business ,with elaborated patterns and acknowledged importance. In the ancient world, however, it was the first cautious stepping into an unknown field, a sublime personal work of pleasure. Empiricism was chiefly the all-day practice (*i.e.,* of sun and moon), which in such personal occupations broadened into more attentively looking at uncommon things (*e.g.,* the planets). Theory was the new world of wonder beyond the common knowledge, it was philosophy searching wisdom, eager to discover the essence of things and to unveil world-structure.

So observations in ancient science had no protocol-character; the idea of errors of observation as a normal character, determining their treatment and asking for elimination, played no role. When it was necessary one looked or measured where the planet hung out. The figures indicating its position were simple facts. Ever again we read in Ptolemy, after his mentioning the observations: these numbers we accept as given. And then follows the geometrical demonstration, taking a broad space, how to derive from these data by means of exact propositions and computations the desired elements of the orbit. There does not appear any concern about the reliability and accuracy of the figures used or about its influence upon the values derived. Geometric demonstration is paramount, and this supremacy goes so far as to derive correct results by applying a well-devised method to fictitious data. Again Aristarchus' little work may give a hint as to the underlying thought by calling the empirical data at the outset "hypotheses," *i.e.,* assumptions from which the real, the geometrical work has to start.

The outcome of the work is a geometrical picture of the world of celestial bodies. It is a picture of eternal continuous motion in circular orbits, obeying determinate laws, a picture full of simple harmony, a "cosmos," an ornament.* It is expounded in a straight progression of exact demonstrations, without disturbing irregularities. When the line of apsides of Venus is derived from equal elongations at both sides,

*The Greek word "kosmos" means ornament.

these for the strict demonstration had to be exactly equal. Had they been a little bit different, an explanation to the reader would have been needed that it made no essential difference; this digression would have distracted the attention, left a certain feeling of doubt, and disturbed the harmony of the construction. The book, moreover, the work of the author, had to conform to a standard of perfection in form and workmanship. Spiritual work in those times was not so different from handicraft; just as the craftsman removed carefully from his product any rough irregularity that could disturb the eye in enjoying the pure harmony of form and line, so the mental piece of work had to captivate eye and mind in the pure presentation of the universe in its mathematical image.

Geometry in Greek culture occupied a paramount place as the only abstract and exact science. For its rigidly logical structure, proceeding from axiom and proposition to proposition it stood out as a miracle of the human mind, a monument of abstract truths, entirely outside the material world, and notwithstanding its visibility to the eye entirely spiritual. The small bit of utility in its origin from Egyptian geodesy hardly carried weight against Euclid's great theoretical structure. Surely the "sphaerica" too, the theory of the sphere and its circles found a wide practical application in astronomy in the description of the phenomena of the celestial sphere, the risings and settings of the stars, and so was used and taught during all following centuries. But spherics was a small part only of geometry. The entire science of lines and angles, of triangles, circles, and other figures with their relations and properties was a purely theoretical doctrine, studied and cultivated for its inner beauty.

Now, however, came the science of the planetary motions, the work of Ptolemy, as a practical embodiment of the theory. What else would have been imagined truths, existing in phantasy only, here in the structure of the universe became reality. Here it acquired form and specificness, definite value and size. In the world of planets the circles moved, distances stretched and shrank, angles widened and dwindled, and triangles changed their form, in endless stately progress. If we call Greek astronomy the oldest, indeed the only real science of antiquity, we must add that it was geometry materialized; the only field, truly, where geometry could materialize. Whereas without this world the practice for students of geometry would have been restricted to an idle working with imaginary self-constructed figures, they found here a realm, the sole but the grandest, where their figures were real things, where they had definite form and dimension, where they lived their own life, where they had meaning and content, the orbits of the celestial luminaries. Thus the "Mathematical Composition" was a pageant of geometry, a celebration of the most profound creation of human mind in a representation of the universe. Can we wonder that Ptolemy in a

four-lined motto preceding his work says that in studying the orbits of the stars he feels himself sitting at Zeus' table and tasting celestial ambrosia?

When we realize that the geometrical point of view pervades and determines Ptolemy's work entirely, all qualities we found in it, also those at variance with our scientific codes, become intelligible and rational. And unreservedly we can join in the admiration it met with in all the later centuries.

Observations of Saturn, 1943-46

By WALTER H. HAAS

Introduction

This paper will report observations of Saturn carried out by a small group of amateurs between July 18, 1943, and June 22, 1946. All dates and times are by Universal Time. The observations fall in three different apparitions, and the planet went unwatched only for about two months near each intervening conjunction. The dates of opposition are December 16, 1943; December 29, 1944; and January 12, 1946. The oppositional declination δ of Saturn was $+22°$ each time. The oppositional value of B, the saturnicentric latitude of the earth, was $-27°$ in 1943, $-26°$ in 1944, and $-23°$ in 1946. Hence, the planet was well placed for northern observers; and the rings were widely opened. Counting everything noted by one observer on one date as just one observation, our group recorded almost 500 observations.

We reproduce in Figure 1 some of our better views of the planet.

The observers are:

Name	*Telescope*	*Station*	*Remarks*
C. F. Gramm	6-inch refr.	Rochester, N. Y.	University of Rochester Telescope
W. H. Haas	18-inch refr.	Upper Darby, Pa.	Flower Observatory
W. H. Haas	6-inch. refl.	New Waterford, Ohio	
T. R. Hake	5-inch refr.	York, Penna.	
H. M. Johnson	6-inch refr.	Rochester, N. Y.	U.R. Telescope
A. W. Mount	8-inch refl.	Ft. Worth, Texas	
E. J. Reese	6-inch refl.	Uniontown, Penna.	
F. R. Vaughn, Jr.	7-inch refl.	Columbus, Ohio	
E. K. White	two 9-inch refl.	Chapman Camp, B.C., Canada	

South Equatorial Belt

Except for a few months early in 1945, a band near the latitudinal center of the ball was much darker and much more conspicuous than any other belt. When the view was good enough, this S.E.B. was seen

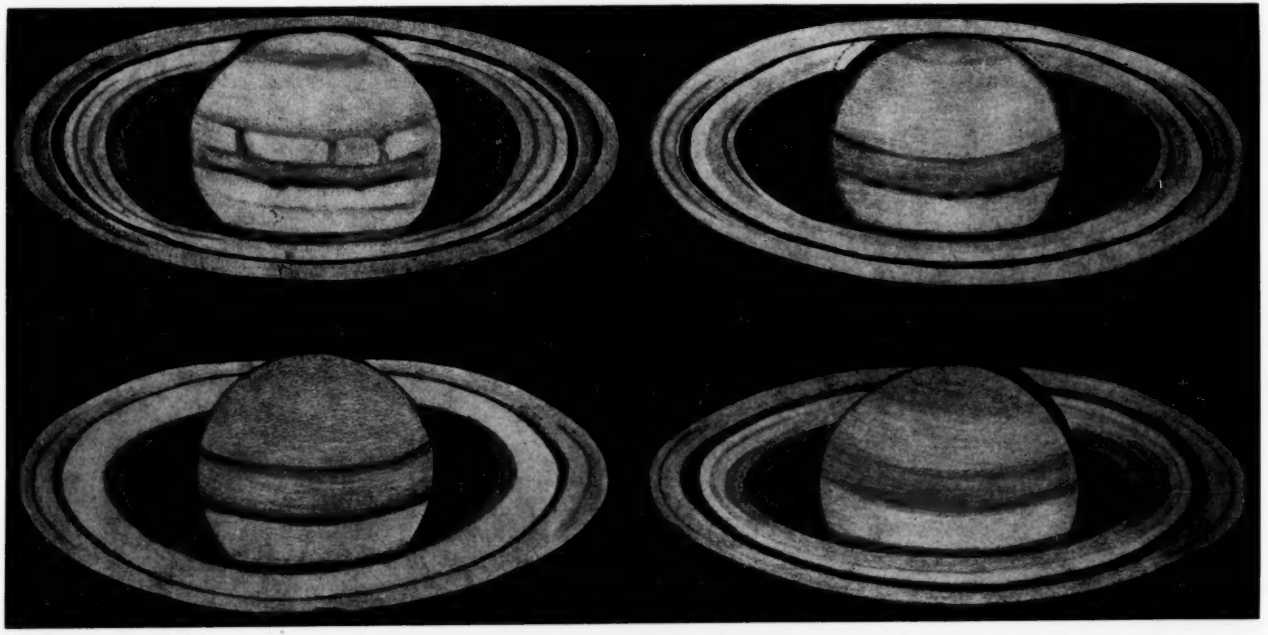

FIGURE 1 DRAWINGS OF SATURN

Upper left—W. H. Haas. January 8, 1944. $0^h\ 43^m$ to $1^h\ 52^m$. 18″ refr. at 300x. Seeing poor to fair. Sky Clear. Position for $1^h\ 23^m$.
Upper right—E. K. White. October 7, 1944. $13^h\ 30^m$. 9″ refl. at 300x. Seeing very good. Sky very clear.
Lower left—A. W. Mount. January 12, 1946. $4^h\ 30^m$. 8″ refl. at 133x and 266x. Seeing good. Sky Clear.
Lower right—E. J. Reese. March 13, 1946. $2^h\ 30^m$. 6″ refl. at 240x. Seeing very good.

double with the space between the components dusky. The north component was usually darker than the south component. This pair of belts showed a reddish or brownish hue to most observers noting their color. With the 18-inch telescope I saw much delicate detail in this doubled belt, such as dark spots in the S.E.B.N, dark humps on its north edge, and dark projections or columns with bases on the south edge of the S.E.B.S. The humps on the north edge were visible to Johnson and White also in good seeing. Similar *delicate* markings have been seen by Lowell[1] and by Antoniadi.[2] The appearance of these belts to us did vary at times from the pattern just described. On October 31, 1944, the 18-inch in good seeing showed the S.E.B. single. On three dates only with this telescope, and in average seeing, I called the space between the components white rather than dusky. On March 13, 1946, Reese found the belt fainter, broader, and more diffiuse than it had looked on February 18, 1946, in much worse seeing.

South Polar Belt

During the summer and early autumn of 1943, this belt was darker than during most of the 1942-43 apparition, the change having probably occurred in April, 1943.[3] On December 5, 1943, I thought it darker still. Nevertheless, White was the only other observer who distinguished this belt from the shaded south polar region; and even he saw it usually only as a slightly darker north edge to that shading. The S.P.B. did have one period of conspicuousness. Between November 26, 1944, and January 21, 1945, this belt became much darker and far more conspicuous than the S.E.B. Compatible observations by White and myself first surely revealed the subsequent fading of the S.P.B on March 12, showed near-equality with the S.E.B. on March 27, and made it much inferior to that belt by May, 1945. The fading may have continued, and probably the S.P.B. was as undistinguished during the 1945-46 apparition as in 1941-42.[3] White and I each saw the S.P.B. to be gray. When conspicuous early in 1945, it was much wider and/or darker in some longitudes than in others; on one date it even looked mottled.

Other Belts

With two exceptions, only the Flower 18-inch revealed belts other than the S.E.B. and the S.P.B., evidence of the advantages of large telescopes in planetary studies.[4]

The most conspicuous of these delicate belts was usually a narrow one located in the bright equatorial zone and lying closer to the north edge of the S.E.B. than to the south edge of the projected Crape Ring. This belt was first suspected on August 23, 1943, and was subsequently usually visible with the 18-inch in seeing not bad so that there is little reason to think that it had just darkened in August, 1943. This equatorial band was often darker in some longitudes than in others. On

September 28, 1943, its color to me was "light copperish gray, as usual." Perhaps our E. B. is Lowell's equatorial "dark chord."[1]

A narrow and usually very faint belt somewhat south of the S.E.B.S as a rule ranked next in order of conspicuousness. First recorded on November 6, 1943, this south temperate belt, as we shall call it, was subsequently often remarked when Saturn was defined well enough and was probably no new feature in November, 1943. This belt too was often darker in some longitudes than in others. It appeared gray but was really too faint to show any other color.

The equatorial band and the south temperate belt varied in conspicuousness, at least relative to each other. In August-November, 1943, the E. B. was the stronger. From December 2 to 19 the S.T.B. was seen clearly while the E. B. was invisible or nearly so. In January-February, 1944, the E. B. was again superior. It was still so in August-September, 1944, when I thought it darker than during the 1943-44 apparition. However, from October 30 to November 9 the S.T.B. was at least at strong as the E. B. Throughout 1945 and into early 1946 the E. B. was the more conspicuous with one recorded exception.

On February 3 and 24, 1945, and on January 28, February 18, and March 4, 1946, I saw with the 18-inch a belt apparently about halfway between the south edge of the S.E.B.S. and the north edge of the S.P.B. On April 19, 1946, White (and C. T. Dakin with him) in poor seeing saw a faint belt perhaps closer to the S.E.B.S. than to the S.P.B. Having often failed with *excellent* views to see any belts but the S.E.B. and the S.P.B., White expresses confidence that a darkening had just occurred. Subsequent observations capable of shedding light on the possible change are unfortunately lacking. Whether these six views refer to a S. T. B. shifted in position or to a fifth belt, a south south temperate belt, is uncertain.

On December 10, 1943, Johnson in average seeing *suspected* the space between the S.E.B. and the S.P.B. of being "finely belted," and with the 18-inch I occasionally *suspected* one or more belts between the S.T.B. and the S.P.B.

Equatorial Zone

The space between the projected Crape Ring and the S.E.B. was always definitely the brightest part of the ball. Its color was white to all the observers.

Other Zones

With the 18-inch I saw all during the 1943-44 and 1944-45 apparitions (no data for 1945-46) the space between the S.E.B.S. and the S.T.B. to be brighter than the space between the S.E.B. components and actually almost as bright as the E. Z.

The ball was dusky south of the S.T.B. (south of the S.E.B., except in the 18-inch) and was capped by a still darker south polar region

shading. The work of Johnson and Gramm indicates that this S.P.R. grew lighter and probably also smaller between December 4, 1943, and January 19, 1944. A Lick photograph on December 14, 1943, shows a remarkably dark S.P.R. shading.[5] Vaughn called this shading faint bluish on May 15, 1946, and faint brownish eight days later. I alone observed the limb to be whitish south of the S.P.B. rather than shaded.

Latitudes of Belts

These were obtained by measures of *original* drawings. The reduction was made with formulas which allow for the oblateness of Saturn;[6,7] they are:

$$\tan B^1 = \sec \epsilon_0 \tan B$$
$$\sin (\beta^1 - B^1) = y/b$$
$$\tan \beta^{11} = \sec \epsilon_0 \tan \beta^1$$

Here $\sec \epsilon_0$ is the ratio of the equatorial and polar radii and is 1.12 for an assumed oblateness of 0.105.[8] The latitudes β^{11} are *saturnigraphic* latitudes. In the table below, column (1) gives the means of measures of seven drawings by White from September 25, 1943, to October 7, 1944. Column (2) is the averages of drawings by Mount on September 10, 1944, and on January 12, 1946. Column (3) is for Reese's drawing on March 13, 1946; column (4), Gramm's on December 4, 1943; and column (5), mine on January 8, 1944. The drawings thus used were made at values of B ranging from —23°.2 to —27°.0. The negative signs mean that the latitudes of the belts are south. I consider the values obtained at least roughly compatible with 1939-43 *saturnicentric* measured latitudes.[3]

	(1)	(2)	(3)	(4)	(5)	Adopted
N. edge S.E.B.	—14°	— 9°	—13°	—14°	—18°	—14°
S. edge S.E.B.	—32	—36	—35	—28	—30	—32
N. edge S.P.B. (or S.P.R.)	—76		—65	—61†	—86	—72
S. edge S.P.B.	—80*					—80
center E.B.					— 8	— 8
center S.T.B.					—37	—37

†Used also a drawing on January 19, 1944.
* Four drawings only.

Crape Ring

All the observers saw the dusky ring readily enough, and the ball was easily visible through it. This ring looked gray to White, light purplish to Gramm on one date, and dark copper-red to me. This redness I noted with both the 6-inch reflector and the 18-inch refractor and confirmed through the use of several Eastman Kodak Company Wratten Filters. The Crape Ring projection on the ball showed darker spots to me, features never seen in the part of the ring off the ball. White on January 21, 1944, observed a very distinct darker spot in the Crape Ring projection. This spot showed no detectable motion for a whole

hour and hence cannot have been really either in the ring or on the ball. Are the spots illusions?

Fifth Division

We use this term to denote a black gap between Rings B and C. White "discovered" it in the autumn of 1943, and I confirmed the feature at the beginning of 1944. It was subsequently regularly visible in good enough seeing to him and me, using the largest telescopes of any of the group; and our work gives every reason to think that this gap is a regular feature of the rings. White thought the fifth division *about* ¼ as wide as Cassini's Division; I, about 17/20 as wide. Perhaps irradiation reduced the true breadth less in the larger telescope. An 1887 drawing by Terby probably shows our fifth division,[9] Lowell recorded it,[8,10] and the Pic du Midi observers saw it in 1943-44.[11]

Ring B

The middle ring was, as a unit, much the brightest part of the Saturnian system. It was brightest of all in its outer third. Two observations in good seeing, one by White and one by Reese, indicate that the outer half or so of this outer annulus was the very brightest part of all. The inner 2/3 of Ring B was dullest at its inner edge, the brightness *possibly* decreasing inward by steps rather than continuously. This ring was white, at least in its brighter parts, to all observers noting its color. Johnson once thought the inner portions comparatively yellow.

Third Division

"Discovered" in 1942,[8] this concentric shading in the rings was regularly visible to White and me in 1943-46, except in the worst seeing. The feature was apparently unchanging, and there is thus every reason to regard it as a normal part of the ring-system. Hake confirmed this object once with the 18-inch and once with his 5-inch in near-perfect seeing, I saw it clearly with my 6-inch reflector and with a 10-inch refractor as well as with the 18-inch, and Gramm and Reese perceived it imperfectly as a shading near the inner edge of Ring B. This third division was *never* seen as a black gap; it was instead dusky, probably darker than the S.E.B. White and I agree that this division is comparable in width to Cassini's, being perhaps as much as 5/4 as wide as that gap. White found from measures of three good drawings that the center of the third lies 0.16 of the way from the inner edge of Ring B to the outer, and I obtained 0.19 ± 0.01 (probable error of mean) from eight visual estimates at the telescope. This division appeared gray to White.

Fourth Division

Though "discovered" in 1942,[8] this *delicate* concentric shading still much needs further confirmation. During the three apparitions being discussed I saw it clearly eight times with the 18-inch only, and White

suspected it twice. Yet White repeatedly failed to see this mark during his best views of Saturn, and the remaining observers never caught a glimpse of it. The fourth division lies at the inner edge of the bright outer third of Ring B. It is perhaps 1/3 as wide as Cassini's, and it is probably extremely faint.

Cassini's Division

Everyone easily saw this famous black gap all the way around the rings (except behind the ball).

When Cassini's Division was almost tangent to the south limb in 1943-44 and 1944-45, I saw it widen into a wedge-shaped notch where it touched either limb of the ball, clearly an optical effect.

During parts of 1943 and 1944 White, Mount, Johnson, and I observed the middle of Cassini's Division, or even its inner edge, to appear exactly tangent to the south limb of the ball. We saw this effect at values of B ranging from —25°.1 to —26°.9. Now on March 17, 1944, with B = —26°.95, the polar radius r was 8″.21.[12] It can be shown that r and the radius r′ passing through the earth-turned pole on the disc are related by the approximate formula:

$$r' = \sqrt{[(1 + \cot^2 B)\, r^2/(.801 + \cot^2 B)]},$$

where .801 is a factor corresponding to an assumed oblateness of Saturn of 0.105. This approximate formula underestimates r′ by an amount of at most 0″.04 in the places where it has been used in this paper. Uncertainties in oblateness[2,8] may well cause larger errors. Though *A. E. N. A.* is based on an oblateness of about 1/9.5,[12] G. M. Clemence kindly informed me by letter on March 6, 1947, that a value of 1/9 is not completely ruled out. The formula makes r′ = 8″.38 on March 17, 1944. The minor axis of the outer edge of Ring A was then 18″.73.[12] The use of the proper factors[12] gives 8″.15 for the semi-minor axis of the center of Cassini's and 8″.24 for the semi-minor axis of its outer edge. Hence, on March 17 the south limb was really projected on Ring A; and the amount by which it projected on to Ring A was larger at smaller numerical values of B. The *apparent* tangency of the south limb and Cassini's was thus an illusion. Now this same near-tangency is to be seen on a Lick photograph for December 14, 1943;[5] and the *extreme* darkness of the S.P.R. shading on the photograph certainly makes it easy to suppose that the *true* south limb is there invisible and perhaps was also invisible visually near that date. It may well be that even a faint S.P.R. shading or S.P.B. can darken the limb enough to make the ball appear too small and thus cause *seeming* tangency near the largest attained values of | B |.

When the rings are opened to their maximum extent Cassini's has been reported present above the globe by many past observers.[13] The illusion is thus a common one—*unless* we have an effect of small errors in some *Nautical Almanac* quantities.

Ring A

The outer ring was much duller than the middle ring. *As a whole*, the outer ring to me in August-December, 1943, and in September, 1944, was about as bright as the E. Z. However, Johnson on January 19, 1944, thought it only a trifle brighter than the S.P.R. shading and presumably dimmer than the E. Z. In late October and in November, 1944, I found Ring A duller than the E. Z. and about equal to the space between the S.T.B. and the S.P.B. By February-April, 1945, it was even duller than that space. A few data suggest additional variations during the 1945-46 apparition. The observations appear to show clearly that either Ring A or much of the ball varied in brightness. There is good empirical evidence that changes of this sort in the ring-system do occur,[5,13,14] although they appear hard to explain for physical reasons.

When conditions were favorable enough, White and I regularly saw Ring A to be somewhat brighter outside Encke's Division than inside it. Hake suspected this same difference on one near-perfect night.

This ring was bluish to Vaughn, bluish-gray to Reese, gray to White, and dull white to me.

White and I repeatedly observed what we have every reason to suppose to be a normal feature of the rings, namely a narrow bright annulus at the extreme inner edge of Ring A. This annulus was much the brightest part of A, though perhaps actually no brighter than the inner edge of Ring B. The width of the annulus was estimated as 1/3 to 1/2 that of Cassini's Division. I saw this annulus with a 6-inch reflector and a 10-inch refractor as well as with an 18-inch refractor. Dawes, Coolidge, and Lassell have remarked a greater brightness of the inner edge of A[14]. However, past observers have also recorded many features in the rings which our work has *not* confirmed. A more conclusive fact is that this annulus appears to be present on 1939 and 1943 Lick photographs.[5]

On February 25, 1944, White suspected a bright annulus midway between Encke's and the outer edge of A, a feature that he later failed to see. With excellent conditions on November 1, 1944, I remarked the outermost tenth of Ring A to be brighter than all the rest of it but the inner annulus. I confirmed this appearance during good views on November 3 and 14 but could perceive nothing of it in good to excellent seeing on February 25 and 26, 1945. White at least three times found the outermost eighth or so of Ring A to be darkened. These *possible* features outside of Encke's *require* further study; a large telescope, good seeing, and a power of 400X or more appear necessary.

Encke's Division

This concentric shading in the rings was seen at least once by every observer but Vaughn, who observed just a few times with poor con-

ditions. They all agree that Encke's lies near the middle of Ring A, 13 estimates by me placing it 0.52 ± 0.01 (probable error of mean) of the way from the inner edge of A to the outer. They further agree on its narrowness, White making it about 2/3 as wide as Cassini's and I obtaining 0.70 ± 0.03 for this ratio from 10 estimates. At times Encke's looked to me *as if* it might be observed as a black gap in good enough seeing; but such a character was never clearly revealed, even in the 18-inch. The other telescopes often showed this feature as little but a faint grayish line. I found Encke's usually definitely darker and thinner than the third division described above. In September-October, 1944, however, I saw Encke's to be abnormally faint and lighter than the third, an aspect present then even in splendid seeing. The recovery of "normal" darkness occurred between October 31 and November 9, 1944. This division appeared gray to White.

Saturnian Shadows

The shadow of the rings on the ball was unobservable north of the ring-ellipse during the years under discussion except that it prevented the ball from being seen there during the first half of the 1945-46 apparition. Though shadow must often have existed on the south edge of the ring-ellipse, we quite failed to distinguish such shadow from the Crape Ring projection.

Except near an opposition, the shadow of the ball on the rings was prominent. Several observers agree that during 1943-44 and 1944-45 this shadow was present on Ring B only, an aspect confirmed by mathematical calculation, and that its bounding edge in part *appeared* to follow Cassini's Division (see Figure 1). The latter effect was surely an illusion. In 1945-46 the shadow was correctly seen across both Rings A and B. It then largely lacked anomalies of the sort sometimes recorded in the past.[3,14]

On the Visibility of Thin Dark Bands

The question of how narrow a black band can be seen against a bright background obviously has wide implications in the field of lunar and planetary observations. W. H. Pickering reported from a study of an artificial disc that a line of width 0".07 is clearly visible.[15] Studies of the shadow of the ball of Saturn near opposition have the advantage that one is dealing with a real planetary feature under the actual conditions of observation. The width of the shadow can be computed as follows:

Consider a plane perpendicular to the line of sight from the earth to Saturn and at a distance R from the center of Saturn. If that assumedly spherical planet has a radius ρ, then the radius of the circular (ignoring foreshortening) cross-section of shadow in our plane is also ρ to within an error never exceeeding 0.8%, and usually far less than that, for values of R less than the radius of the outer edge of Ring A. In

this same plane the distance of the center C′ of the circular cross-section of the shadow from the projected position of the observed center of the disc of Saturn is R tan i, where i is the angular separation of the sun and the earth as seen from Saturn. On spherical Saturn let S be the south pole, let S′ be the sub-sun point, and let E′ be the sub-earth point. Then, with *A. E. N. A.* notation,[12] arc SE′ = 90° + B, arc SS′ = 90° + B′, arc S′E′ = i, and ∠ S′SE′ = | U′ + ω — U |. Hence:

$$\cos i = \sin B \sin B' + \cos B \cos B' \cos | U' + \omega - U |.$$

If ∠ SE′S′ is called ϕ, one has:

$$\sin \phi = (\cos B' \sin | U' + \omega - U |)/\sin i.$$

Now project spherical triangle SE′S′ upon the reference plane described above. Choose a y-axis through E′S (projected), positive to the south; and choose an x-axis perpendicular to the y-axis at E′ and positive toward the following limb of Saturn (right in inverting telescope). The coördinates in this plane of the C′ mentioned above will be numerically: (R tan i sin ϕ, R tan i cos ϕ), where x_c takes the sign of (U′ + ω — U) and y_c is positive only when sin B′ > sin B cos i.*

In our plane the projected limb of Saturn is an ellipse of semi-minor axis (nearly) r, the polar radius, and semi-major axis r/(1 — k), where k denotes the oblateness of Saturn and is again taken as 0.105. Setting r = 1, the equation of the limb is:

$$x^2 (1 - k)^2 + y^2 = 1.$$

The locus of points in the rings, again as projected upon our plane, at a distance R from the center of Saturn is an ellipse of semi-major axis R and semi-minor axis R sin B and has the equation:

$$x^2 \sin^2 B + y^2 = R^2 \sin^2 B.$$

A simultaneous solution of these equations will supply the coördinates of a point of intersection P_1 of the two ellipses. One chooses x_1 to agree in sign with (U′ + ω — U) and y_1 to be positive when B is negative. One next computes the radius ρ of the planet at P_1 as:

$$\rho = \sqrt{x_1^2 + y_1^2}.$$

The equation of the edge of the shadow is a circle with radius ρ and with center at C′. The width W of the shadow is logically defined as measured along radius $C'P_1$. Hence:

$$C'P_1 = \sqrt{(x_c - x_1)^2 + (y_c - y_1)^2},$$

and $W = \rho - C'P_1$.

To change W to angular measure, one multiplies by the polar radius r in angular measure. We used R = 1.8r in our studies and thus obtained the breadth of the shadow near the middle of Ring B.

*For the subscript c in x_c and y_c in this sentence and in the fourth equation following *read* c′.

There are available for study 61 near-opposition observations of the shadow of the ball, chiefly by White with a 9-inch reflector and by me with an 18-inch refractor. The shadow has always been seen and has been recognized as shadow when its width is 0″.20 or more. The narrowest shadow so recognized, by White on January 5, 1946, had a breadth of only 0″.15; on two other occasions White did not recognize shadows of widths 0″.15 and 0″.17, respectively, to be such. No observer has ever recorded a *black* shadow when its width is 0″.13 or less; rather one then sees a narrow and notably dark band adjacent to the limb on each arm of the rings or else a single band outlining the whole south (north for $B > 0$) limb when $|B|$ is large enough. Of course, shadows of widths 0″.20 or more cannot be really totally black; I write of telescopic *impressions*. The dark band on the shadowed arm appears to be wider than the dark band on the unshadowed arm with the true shadow as narrow as 0″.09, perhaps much narrower. (With a 6-inch reflector, I found the band on the shadowed ansa still looking wider than the other band only 46 hours before the 1947 opposition.) The false shadow just described is present on *good* published photographs, [5,16] which *appear* to show shadow on both limbs near opposition.

With apertures of 18 inches and less, diffraction would presumably make the telescopic image of the shadow have a width of more than 0″.20 so that a lightening of the shadow should have been observable for larger breadths. *Perhaps*, however, the overlying of the diffraction-pattern of the shadow by the diffraction-patterns of neighboring much brighter points substantially reduced the effective width of the shadow below its theoretical value. If so, we may have an explanation of how the observers could make comparisons of dark bands (and of annuli and divisions in the rings) of breadths less than 4″.5/a, where a is the aperture of the telescope in inches. But again, if diffraction enlarged to the same width two bands of equal intensity but of different true widths, the wider would be seen the more intense and might *seem* wider for that reason. The replacement of "black" shadow by a dark band when the width is below 0″.15 and the presence of a similar band on the ansa free from shadow are surely illusions, probably due to contrast in brightness between the ball and the rings. These phenomena have been found so alike with 9-inch and 18-inch telescopes that contrast-illusions are a better explanation than diffraction-effects. The penumbral shadow beside the umbral shadow can play only a very minor role, for its width is less than 0″.03.

The contrast-band, if such it be, on an unshadowed arm of the rings is perhaps most conspicuous at opposition, though Hake once recorded it 40 days after an opposition. Its width must be very small; with the 18-inch I described it as much narrower than the true shadow on January 1, 1944, when the width of the latter was only 0″.37.

White Spots on Rings

We have already published on a curious white spot on the rings adjacent to the shadow of the ball and on a similar spot adjacent to the limb on the unshadowed arm of the rings.[3,17] It is my opinion now that these white spots are merely contrast-effects, the result of the differing brightness of adjacent areas. Photographs of Saturn show no trace of them,[5,8,16] including some *original* Lowell Observatory ones kindly exhibited to me during a visit there in September, 1946, by Messrs. E. C. Slipher and C. O. Lampland. These Lowell pictures are so excellent as to reveal belts between the S.E.B. and the S.P.B.

Nevertheless, contrast-effects may be *important* in general lunar and plantary studies; and I should hence like to put on record the following things about these white spots:

1. To White and me these objects looked *diffuse*, and I was much reminded of presumably similar contrast-whitenings observed beside some lunar shadows.

2. When the shadow of the ball existed on Ring B only in 1943-44 and 1944-45, the white spot beside it was seen on Ring B only. The same may well have been true of the spot on the unshadowed ansa. Was Ring A too dull to provide the degree of contrast needed to produce these illusions?

3. White looked vainly for these spots up to March, 1944. He then started seeing them and subsequently almost always did so. Hake found the spot beside the shadow *easy* when I personally directed his attention to it in April, 1944; but during 1944-45 this spot was only intermittently visible to him, and he saw no spots at all with a near-perfect view two days before that opposition. Mount and Reese apparently never caught a glimpse of these features. Vaughn, Gramm, and Johnson, who had all recorded the spot(s) at earlier apparitions, saw them at least at times during the period under discussion. White writes that visitors to his private observatory saw the spots *after* he pointed out these objects to them. I regularly saw the spots; but they fluctuated from extreme vagueness to striking plainness, variations that appeared to depend singularly little upon seeing, transparency, brightness of sky, and even aperture. I have sometimes seen them well with the 18-inch in excellent seeing.

4. Within roughly 10 days of an opposition the two spots looked very similar in size, brightness, and general appearance. Farther from opposition the one beside the shadow was usually larger and brighter than the one beside the spurious dark limb band described above. I have seen the spot on an unshadowed ansa as much as 60 days from an opposition, and White has recorded it as very narrow and diffuse near quadrature.

5. A few estimates by Hake and Johnson make the white spot be-

side the shadow of the ball about as large as that shadow. White and Vaughn (one estimate by latter) made it one to two times as large as the shadow. I obtained one to three for this ratio, with values larger than three when the shadow was small near opposition and also near conjunction. The actual width to my eye was probably about 3″. White reports that the near-opposition spots looked smaller than the spot beside a large shadow far from opposition.

These anomalies in appearance are what one might expect of illusory contrast-effects, which must be very different for the eyes of different observers.

Phase of Saturn

White and I, with the largest telescopes used by the group, frequently saw the limb of Saturn to be slightly brighter than the terminator when observing conditions were favorable near a quadrature. Many years ago W. H. Pickering noted the dullness of the terminator near quadrature with a 13-inch refractor;[15] and W. W. Leight of North Hills, Penna., has seen the phase in this same way with an 8-inch reflector.

On the Near-Tangency of Rings and Ball

During the 1945-46 apparition the outer edge of Ring A was almost exactly tangent to the south limb of the ball. White and I made 37 estimates of whether the limb projected above the rings or whether the reverse was true. One can extract the semi-minor axis of the ring-ellipse from tables, and one can find the radius r′ of the disc passing through the visible south pole by the formula given above. The observed appearances recorded by me, not only with the 18-inch but also occasionally with 4- to 6-inch telescopes, agreed remarkably well with these computations; the ball and the rings were called sensibly tangent only when neither projected above the other by as much as 0″.1, and otherwise the one which projected was correctly described—all supposing the computed values to be exactly correct. Both with the 18-inch refractor and with the 6-inch reflector, I thought a projecting strip of ball easier to see when of width near 0″.5 than when of width near 0″.2. At least with the 6-inch, diffraction should have enlarged both to the same size; perhaps the wider strip was easier because its telescopic image was brighter. With a 9-inch reflector White saw the limb to project above the rings when the amount of the projecting was between 0″.3 and 0″.6 but could usually see no difference from exact tangency when the amount of the projecting was 0″.3 or less. Was the true south limb invisible to him, as it apparently was to me with a 6-inch telescope in 1941-42?[23]

References

[1] P. Lowell, "Our Solar System," *P. A.*, **24**, 419, 1916.
[2] E. M. Antoniadi, "Observations of Saturn in 1936-1937 . . .," *J. B. A. A.*, **47**, 252, 1937.

[3] W. H. Haas, "Observations of Saturn, 1939-43," *J. R. A. S. C.*, **38**, 177, 1944.
[4] W. H. Haas, "The Superiority of Large Telescopes for Visual Lunar and Planetary Observation," *Sky and Telescope*, **4**, No. 4, p. 16, 1945.
[5] M. A. Ainslie, "Photographs of Saturn by the Lick Observatory," *J. B. A. A.*, **55**, 125, 1945.
[6] A. Marth, *M. N. R. A. S.*, **56**, 533, 1896.
[7] A. C. D. Crommelin, *M. N. R. A. S.*, **61**, 114-6, 1900.
[8] H. N. Russell, R. S. Dugan, and J. Q. Stewart, *Astronomy*, Volume 1, 1926.
[9] W. F. Denning, *Telescopic Work for Starlight Evenings*, p. 203, 1891.
[10] P. Lowell, "Memoir on Saturn's Rings," frontispiece drawing, *Memoirs of the Lowell Observatory*, **1**, No. 2, 1915.
[11] B. Lyot, "Planetary and Solar Observations on the Pic du Midi in 1941, 1942, and 1943," *ApJ.*, **101**, 255, 1945.
[12] *The American Ephemeris and Nautical Almanac*, Volume for 1944.
[13] R. A. Proctor, *Saturn and its System*, Second Edition, 1882.
[14] T. W. Webb, *Celestial Objects for Common Telescopes*, Volume 1, "Saturn," pp. 204-221, Sixth Edition, 1917.
[15] W. H. Pickering, *Harvard Annals*, **32**, part 2, 1900.
[16] *Sky and Telescope*, **4**, No. 2, back cover photograph, 1944.
[17] H. M. Johnson, "The White Spot on Saturn's Rings," *J. B. A. A.*, **51**, 309, 1941.

INSTITUTE OF METEORITICS, UNIVERSITY OF NEW MEXICO,
ALBUQUERQUE, NEW MEXICO, MARCH 16, 1947.

The Planets in December, 1947

By RAYMOND H. WILSON, JR.

NOTE: The time employed is Central Standard Time unless otherwise indicated. The phenomena have been chosen and described for the North American continent, and especially for the United States. The basic data have been taken principally from the *American Ephemeris and Nautical Almanac*.

Sun. The sun spends most of this month within a degree of its southernmost possible declination of 23½ degrees south. The date of this extreme declination, that is, the winter solstice, is December 22 at 11 A.M.

Moon. The phases of the moon will occur as follows:

Last Quarter	December	4	7 P.M.
New Moon		12	7 A.M.
First Quarter		20	11 A.M.
Full Moon		27	2 P.M.

Perigee occurs on December 28.

An occultation of η Leonis in The Sickle may be seen on December 30, shortly after 11 P.M.

Evening and Morning Stars. Venus will be visible in the evening twilight, while Saturn and Mars will be conspicuous in the morning sky.

Mercury. For a few days near the beginning of the month Mercury might be seen rising about an hour ahead of the Sun, but, since this difference decreases daily, the planet soon becomes invisible.

Venus. This most brilliant planet is now handicapped in its conspicuousness by its very unfavorable position low in the southwest evening sky. However, it remains above the horizon up to 2 hours after sunset.

Mars. As a bright red star in Leo, just east of Regulus, Mars will be very conspicuous in the morning sky. It will rise at about 11 P.M.

Jupiter. Since it is in conjunction with the Sun on December 1, Jupiter will be invisible until toward the end of the month, when it might be seen low in the southeast morning twilight.

Saturn. This planet continues its close association with Mars and the star Regulus in the morning sky, being nearly stationary just a few degrees west of the latter. It will rise at about 10 P.M.

Uranus. Uranus continues to move slowly westward, at a position about one third of the distance from ζ to β Tauri.

Neptune. Neptune will be moving southeastward, at some 3 degrees south of γ Virginis.

Department of Mathematics, Temple University, Philadelphia, Pa.
October 1, 1947.

Occultation Predictions for December, 1947

(Taken from the *American Ephemeris*)

Date 1947	Star	Mag.	IMMERSION Greenwich C.T. (h m)	IMMERSION a (m)	IMMERSION b (m)	IMMERSION Angle E from N (°)	EMERSION Greenwich C.T. (h m)	EMERSION a (m)	EMERSION b (m)	EMERSION Angle E from N (°)
Occultations Visible in Longitude +72° 30′, Latitude +42° 30′										
Dec. 2	λ Canc	5.9	2 25.3	−0.2	+0.8	108	3 20.4	−0.2	+1.5	260
4	46 Leon	5.7	9 58.9	−0.8	−2.6	162	10 58.5	−2.3	+0.1	263
7	46 Virg	6.1	8 14.2	−0.2	−0.8	152	9 10.8	−1.1	+1.2	273
7	48 Virg *m*	6.5	10 0.0	−1.2	+0.6	105	11 8.3	−0.8	−1.1	329
10	11 H.Libr	5.5	11 10.5	−1.0	+1.5	87	12 7.6	−0.2	−0.7	332
19	336 B.Aqar	6.5	23 39.4	−4.3	−3.4	119	0 10.2	..	..	164
25	13 Taur	5.5	6 46.4	−0.6	−1.2	88	7 46.5	−0.4	−0.6	245
25	14 Taur	6.3	7 30.1	−0.1	−2.3	115	8 16.1	−0.4	+0.3	219
25	67 Taur	5.4	21 50.7	−0.3	+1.3	87	22 45.0	−0.1	+2.0	227
25	κ Taur	4.4	21 51.4	−0.1	+1.6	68	22 49.6	−0.4	+1.7	246
31	η Leon	3.6	5 45.7	−1.4	+0.4	105	6 58.2	−1.5	−0.8	305
Occultations Visible in Longitude +91° 0′, Latitude +40° 0′										
Dec. 2	28 Canc	6.1	5 57.7	..	..	17	6 10.2	..	..	357
7	46 Virg	6.1	8 17.2	+0.1	−2.4	174	8 49.7	−0.6	+3.1	243
7	48 Virg *m*	6.5	9 44.4	−0.4	−0.1	132	10 50.6	−1.0	+0.3	295
19	336 B.Aqar	6.5	22 45.9	−2.3	+0.8	79	0 2.5	−0.9	+1.7	204
25	13 Taur	5.5	6 32.7	−1.3	−1.8	103	7 33.4	−1.0	+0.6	223
25	14 Taur	6.3	7 37.5	..	..	153	7 52.0	..	..	177
25	67 Taur	5.4	21 47.5	+0.3	−1.3	72	22 39.5	+0.1	+1.5	246
25	κ Taur	4.4	21 50.7	+0.4	+1.6	53	22 40.8	−0.1	+1.3	265
31	η Leon	3.6	5 24.1	−0.9	+0.4	115	6 31.8	−1.3	+0.4	286
Occultations Visible in Longitude +120° 0′, Latitude +36° 0′										
Dec. 9	α Libr	2.9	15 32.6	−1.6	+0.2	109	16 49.3	−1.2	−1.2	323
15	248 B.Sgtr	5.6	0 10.5	−2.1	−2.1	99	1 22.5	−0.5	+0.1	228
18	161 B.Capr	6.4	2 4.8	+0.5	+2.8	0	2 50.5	−2.9	−2.5	292
19	τ Aqar	4.2	2 22.3	−2.9	−1.1	97	3 21.0	+0.1	+2.2	186
25	13 Taur	5.5	5 38.8	−2.6	−0.5	97	6 47.1	−1.5	+2.2	211
30	90 H^1 Canc	6.1	8 2.2	−1.5	+0.8	91	9 15.1	−1.8	−1.0	305
31	η Leon	3.6	5 9.6	0.0	+0.5	110	6 5.7	−0.3	+0.8	281
31	42 Leon	6.1	13 31.5	−0.8	−2.2	144	14 37.0	−1.2	−1.4	283

Date 1947	Star	Mag.	IMMERSION Greenwich C.T.	IMMERSION a	IMMERSION b	IMMERSION Angle E from N	EMERSION Greenwich C.T.	EMERSION a	EMERSION b	EMERSION Angle E from N
			h m	m	m	°	h m	m	m	°
	OCCULTATIONS VISIBLE IN LONGITUDE +98° 0′, LATITUDE +30° 0′*									
Dec. 2	28 Canc	6.1	5 22.7	—0.1	+1.9	63	6 18.0	—1.4	—0.4	306
2	υ^1 Canc	5.7	6 41.8	—0.9	+2.0	64	7 43.9	—1.9	—1.2	312
2	υ^2 Canc	6.4	7 32.8	—1.3	+3.2	50	8 23.3	—2.1	—2.8	331
7	48 Virg m	6.5	9 57.0	+0.3	—3.8	186	10 28.5	—1.8	+3.5	242
18	161 B.Capr	6.4	2 11.3	—1.0	—0.1	65	3 20.5	—0.2	+0.3	230
19	336 B.Aqar	6.5	22 19.3	—2.6	+1.0	85	23 34.3	—0.9	+2.6	197
26	284 B.Taur	6.0	2 35.8	—1.5	+1.2	74	3 52.0	—1.7	+1.7	236
26	95 Taur	6.2	6 52.8	—2.0	+2.0	39	7 52.3	—1.3	—2.4	300
29	ω Canc	5.9	4 43.6	—0.5	+3.7	38	5 24.5	—2.3	—2.7	329
29	4 Canc	6.2	4 54.5	—1.5	+0.2	106	6 8.3	—1.9	+0.9	265
29	λ Canc	5.9	14 21.4	—0.8	+0.4	49	14 46.5	+1.2	—2.7	349
30	90 H^{5}Canc	6.1	8 44.6	—2.3	—0.3	101	10 2.4	—1.5	—1.9	311
31	η Leon	3.6	5 22.7	—1.0	—1.6	152	6 10.6	—1.0	+2.4	245
31	42 Leon	6.1	14 0.7	—0.7	—1.6	118	15 0.9	—0.2	—1.6	304

*Computed by Edgar W. Woolard and Paul Herget; communicated by Commodore J. F. Hellweg, Superintendent U. S. Naval Observatory.

The quantities in the columns a and b are given for the purpose of making these predictions useful for any place within 200 miles of the point indicated. The procedure is as follows: Subtract the longitude of the point given from the longitude of the place in question; multiply the result in degrees, taking the signs into account, by the quantity under a for the star to be observed; similarly, with the latitude, using b; apply the sum of the products, with its proper sign, to the Greenwich C.T., and obtain the predicted Greenwich Civil Time for the phenomenon at the place of observation. To obtain Eastern Standard Time it is necessary to subtract five hours; Central Standard Time, six hours, etc.

METEORS AND METEORITES

Meteor Notes from the American Meteor Society

By CHARLES P. OLIVIER, President

It is with much pleasure that I substitute for the usual type of Meteor Notes written by myself an article just received from one of the most eminent authorities on meteoric astronomy, Professor Cuno Hoffmeister of Sonneberg, Germany. This was sent to me with the request that it be printed in some American scientific journal and, as it is of special interest to members of the American Meteor Society, this is the logical place for it. For those unacquainted with Hoffmeister's work, I should like to suggest that they obtain, if possible, a copy of his reference book "Die Meteore" which was published in 1937 in Leipzig. Its text is in German. It should be read by everyone interested in meteors. He has also published very numerous articles on both theoretical and practical phases of the subject, most of which appeared in German publications during the past three decades.

This year I published the résumé of the reports on the Perseid meteors sooner than usual. Since sending the paper large numbers of other reports on the same stream have arrived, so that a supplementary paper will be necessary. This

will be somewhat delayed, however, as there is no special reason for hurry in this connection. Despite the summer's activity in observing, very many of our members seem totally idle; they do not even get a single fireball report in for a whole year. Such persons are urged to undertake something useful to justify their membership.

The Ecliptical Meteoric Currents

By C. Hoffmeister

Being engaged in a thorough investigation on meteoric currents the author had to deal with a group having their radiants near the ecliptic and showing partly a shape of appearance strongly different from that of ordinary cometary currents. The main representatives of this group are the two systems radiating the one from Scorpius-Sagittarius in May to July, the other from Aries-Taurus in September to November. The leading features of their appearance are the extremely long duration, nearly 3 months in the two cases just mentioned, and the diffuseness of the radiants, concentrated only at the times of maxima, but spreading over an area up to 30° in diameter at other times. For none of these, partly very conspicuous currents, was the parental comet known, nor could it be found at a recent close examination. Now it must be regarded as a well supported fact, that there is a so far unknown component of a planetary character active in the meteoric phenomenon, having strong relations to the System of Minor Planets.

No general method has been at our disposal up to the present to determine the orbital elements, especially eccentricity and semi-major axis of a meteoric current, when neither the parental comet had been found, nor had there been determined accurate individual velocities of single meteors by special methods. The author has developed new methods to find these orbital elements free from any hypothesis.

One method, applied for the first time in the year 1915, is liable to such strong systematic errors that it could not be used really successfully without some important improvements. Let the apparent radiant of a current be known, and let l be the length in degrees of the path of a meteor, l_1 the angle between radiant and point of apparition, h the altitude of the point of disappearance above the horizon, H the real height of the point of disappearance in kilometers, ϵ the geocentric angle between the place of observation and the ending point of the meteor's path, then the length L of the meteor's path in kilometers is obtained by the formula

$$L = H \times \cos \epsilon/2 \times \operatorname{cosec} (h + \epsilon/2) \times \sin l \times \operatorname{cosec} l_1,$$

and the meteor's velocity by $v = L/D$, D being the observed duration. If, by observations from one place only, the individual heights are unknown, the mean height $\overline{H}$, in the case of a current, may be used, by which means also mean values of L and v can be computed. But, as mentioned above, the systematic errors of observation are so large that the method is not applicable in its plain form, and the alternative of determining the errors by means of currents with known velocities meant in most cases an extrapolation, as long as only the old cometary currents were at our disposal.

The author, therefore, searched for another possibility and disclosed an ob-

viously fairly strong correlation between geocentric velocity and mean duration of meteors, not sensibly affected by physical differences or other influences. Besides this, two very important currents of low velocity became available, one being the well-known current of Draconids of October 9, 1933, the other being the temporary current of the Corvids, observed by the author in South Africa in 1937, the origin of which can be traced with high probability to the periodic comet Tempel 3-Swift, according to the author's recent investigations. Thus the relation velocity to duration could be verified by the formula

$$D = +0.5190\ (\pm 0.0011\ \text{m.e.}) - 0.6925\ (\pm 0.0033\ \text{m.e.})\ \log v,$$

where D is the observed mean duration reduced to a brightness of the meteors of 3^m0. It must be borne in mind that the coefficients of this formula are only valid for the author's observations.

The addition of the Corvids to the list of currents with known velocity also made possible a reliable gauging of the original methods. Thus it was found that the velocities computed by this method are exaggerated by a factor of no less than 2.28, the cause being probably a nearly constant additional error in the observed path lengths in connection with a rather unfavorable transfer of this error to the result. Of course, there occur also a number of meteors not really belonging to the current, especially such with hyberbolic velocities. In order to avoid any arbitrariness in rejecting, no mean values have been used, the maxima of the distribution curves of computed velocities being adopted as equivalents of velocity. The curve representing the relation between this equivalent and the real velocity, obtained by means of the cometary currents, thus allowed me to determine the velocity of the currents, the character of which was unknown so far. The velocity being found, the determination of the orbital elements offers no more difficulties. Here follows the list of elements of the 6 principal Ecliptical Currents:

		Virginids	Sco-Sgr-Syst.	δ Aquarids
Argument of perihelion	ω	286° 25′	105° 27′	146° 57′
Ascending node	Ω	13 0	263 0	310 0
Inclination	i	1 56	6 1	23 44
Eccentricity	e	0.6864	0.9339	0.9264
Semi-major axis	a	1.5261	1.7710	1.5995
Perihelion distance	q	0.478	0.471	0.118
		Piscids	Taurids	Geminids
Argument of perihelion	ω	296° 13′	290° 31′	330° 31′
Ascending node	Ω	169 0	230 0	260 0
Inclination	i	3 30	0 44	23 29
Eccentricity	e	0.7220	0.7294	0.9318
Semi-major axis	a	1.4319	1.5738	1.4286
Perihelion distance	q	0.399	0.426	0.097

Fig. 1 shows the main orbits, corresponding to the maxima of activity, of the 6 principal currents. Obviously there is a regular pattern followed by the two groups of 3 currents each: a current of rather low frequency and moderate duration at the beginning, then a strong current of very long duration, and last a current with a high maximum and short duration. The latter two currents, the δ Aquarids at the end of July, and the Geminids of December, exhibit a rather "cometary" appearance.

The next step was the computation of the "Limiting Orbits," *i.e.*, those orbits belonging to the very first and the very latest reliable radiant of every long en-

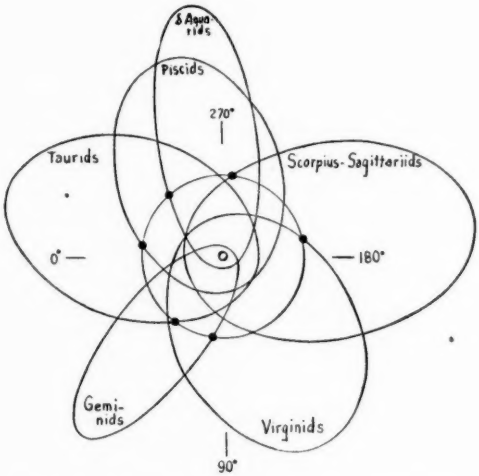

FIG. 1. THE SYSTEM OF THE ECLIPTICAL CURRENTS
Orbits of the 6 principal currents corresponding to times of maxima.

during current. The character of these orbits may be seen from Fig. 2 and Fig. 3. The changes of the elements in the course of the activity of the current are a mere consequence of the fact that the turning of the apsidal line is slower than that of the nodal line, $d\pi/d\Omega$ assuming the following mean values:

Scorpius-Sagittariids + 0.93, Piscids + 0.58, Taurids + 0.34.

In the case of the currents with long duration the limiting orbits induce

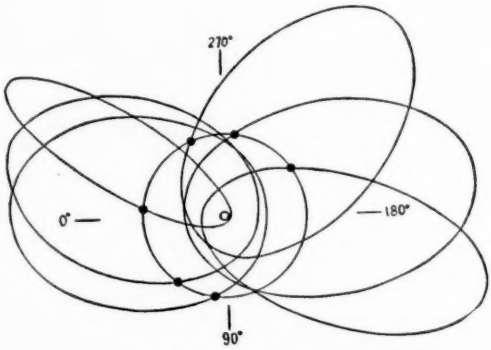

FIG. 2. LIMITING ORBITS
At left Taurids, at right Scorpius-Sagitariids.

us to adopt a system of widespread orbits to which the notion of current cannot be applied in its ordinary meaning. At any rate, the orbits belonging to such a

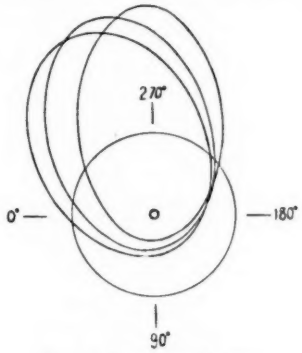

FIG. 3. LIMITING ORBITS
Piscids.

system partly differ so much that they might not be ascribed to the same current when regarded without knowledge of the connecting links.

At an earlier stage of these investigations the author obtained the impression as if there were a permanent radiant active on the ecliptic at about 165° west of the Sun, causing by temporary increase of activity the appearance of currents. But later experiences emphasized the conception that the discrimination of 6 principal currents is justified, not only by the changes of frequency but also by the altitude

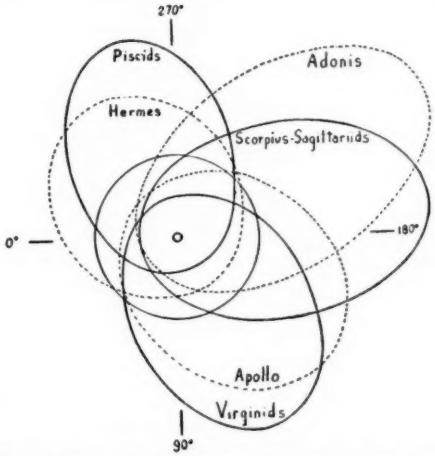

FIG. 4. THE ORBITS OF VIRGINIDS, SCORPIUS-SAGITARIIDS, AND PISCIDS IN THEIR RELATION TO THE ORBITS OF THE MINOR PLANETS, APOLLO, ADONIS, AND HERMES.

of the radiations, the following always appearing 20° to 30° east of the still perceptible vanishing group of the preceding current.

A very strange fact is exhibited by Fig. 4. There is not only a close resemblance between the orbits of 3 ecliptical currents and those of the minor planets, Apollo, Adonis, and Hermes, but the system of the 3 currents of Virginids, Scorpius-Sagittariids, and Piscids nearly covers the system of the 3 planets when turned by an angle of +22° of longitude. One cannot say whether this is a mere random agreement or the manifestation of some nearer relation between the two systems.

The results communicated here have been partly anticipated by Dr. Fred L. Whipple of Harvard College Observatory who, on account of his accurate photographic determination of the velocities of several meteors, suggested the existence of a system of "asteroidal meteors," though, in a later publication, he obviously abandoned these views in general, in favor of another explanation pointing to a rather complicated connection of the Taurids with Encke's comet.

A distinct difference in physical characterization has been found to exist between cometary and ecliptical currents. In the table below the following notations are used:

$\overline{M}$...mean magnitude of all observed meteors.
X...ratio of numbers of observed meteors 4^m and 4^m5 to those of 3^m and 3^m5.
F...ratio of duration of meteors 3^m0 to duration of meteors 4^m0.
T...percentage of trains (observations being confined to meteors brighter than 3^m5).

	Current	$\overline{M}$	X	F	T
		m			%
a.	Cometary Currents				
	Perseids	3.16	0.47	1.14	66
	Lyrids	2.93	0.46	1.20	39
	Leonids	3.24	0.49	1.15	65
	Orionids	3.81	0.76	1.10	81
	η Aquarids	3.18	0.26	1.11	85
	Corvids	2.85	0.54	1.22	71
b.	Ecliptical Currents				
	Virginids	3.91	1.28	1.28	4
	Sco-Sagittariids	3.89	1.20	1.22	26
	δ Aquarids	3.96	1.20	1.30	17
	Piscids	3.92	1.44	1.34	24
	Taurids	3.53	1.10	1.33	23
	Geminids	3.58	0.76	1.30	15

Mean values from preceding table:

a. Cometary Currents
b. Ecliptical Currents

	$\overline{M}$	m.e.	X	m.e.	F	m.e.	T	m.e.
a.	3^m20	$\pm0^m14$	0.50	±0.07	1.15	±0.02	67.8	±6.6
b.	3^m80	$\pm0^m08$	1.17	±0.09	1.30	±0.02	18.2	±3.3

The numbers need no further explanation. They show not only that the two kinds of currents have a different mass-frequency function of the meteorites, but also that the material of the ecliptical currents is more consistent than that of the cometary currents, if it is allowed to interpret the larger share of trains as an indication of increased dissolubility of the matter.

Ecliptical radiants are active all through the year, also between 270° and

360° of solar longitude, when no definite currents can be detected, but the radiants are generally diffuse. Also there are some additional permanent currents with inclinations up to 30° belonging obviously to the ecliptical system. The Hydraids, active in March with a radiant at $\alpha = 180°$, $\delta = -27°$, probably a southern branch of the Virginids, and Denning's α Piscis Austrinids as a branch of the δ Aquarids may be mentioned in this connection.

The total amount of ecliptical meteors compared with the total of observed meteors has been determined to be 16%. The share of cometary meteors is difficult to evaluate. Counts give only 6.5% as belonging to the principal cometary currents, but the rate of trains suggests a much higher share, thus giving indication that there are perceptible rather many cometary stray meteors which have been either separated by perturbations from their original currents or belong to ancient extinguishing currents. It seems, therefore, consistent with all other facts to ascribe to the interstellar component a share of 60 to 70%.

There is strong evidence in the velocity distribution curve of the Scorpius-Sagittariids of an interstellar component being identical with von Niessl's interstellar current and connected to the drift motion found by the author several years ago.

Contributions of The Meteoritical Society

(Known Formerly as The Society for Research on Meteorites)

Edited by FREDERICK C. LEONARD

Department of Astronomy, University of California, Los Angeles 24

A Catalog of the Largest Known Meteorites of the World*

FREDERICK C. LEONARD *and* DOROTHY H. ALLEY
Department of Astronomy, University of California, Los Angeles

ABSTRACT AND INTRODUCTION

Herewith is presented a catalog of the 58 largest known individual meteorites of the world—*i.e.*, those weighing upward of 400 kilograms—enumerated in the order of decreasing weight. The following information concerning each of these masses is given in tabular form: its (1) coördinate number; (2) full geographical name; (3) class; (4) serial number with respect to weight, as a member of the fall to which it belongs; (5) weight in kilograms; (6) time of find *etc.*; and (7) principal depository or depositories (when known). All but 5 of the 58 meteorites listed are siderites; of the 5 non-siderites, 4 are pallasites and 1 is an aerolite (Long Island, Kansas); and *none* of the 58 masses was *observed* to fall.

The second and third paragraphs of the "Abstract and Introduction" to our "Catalog of the Meteoritic Falls of the 11 Western States" (*v. C.M.S.*, **4**, 58; *P. A.*, **55**, 381, 1947) apply with practically the same force to the present catalog, but the following *additional* abbreviations are used in it: cent.—centuries; *i. s.*—*in situ;* men.—mentioned; mn.—main.

Unfortunately, information regarding the weights of a considerable number of the largest known meteorites is woefully inexact—or at best only approximate—as many of the data in this catalog reflect. When weights are quoted in tons by an

*Read in abstract at the 10th Meeting of the Society, 1947 June.

A CATALOG OF THE LARGEST KNOWN METEORITES OF THE WORLD

C.N.	Name	Cl.	Ser. No.	Wt. (Kg.)	Tm. Fd. &c.	Dep.
0179,196	Hoba West, nr. Grootfontein, Southwest Africa	D_1	1	60,000:	Fd. *c.* 1920	*I.s.*; 2kg. Acad. Nat. Sci., Philadelphia
0650,760:	Cape York, Melville Bay, Greenland	Om	1	33,180:	Kn. 1818	Am. Mus. Nat. Hist.
1078,261	Bacubirito, Sinaloa, Mexico	Off	1	*27,000:*	Fd. 1863	*I.s.*
0331,091	Mbosi, Rungwe Dist., Tanganyika Ter., Africa	Om	1	*26,000:*	Fd. 1930	*I.s.*
0350,*080*:*	Bezerros, Pernambuco, Brazil: "prob. mythical"	Si	1	20,000:	Fl. 1915 May 9?	
1226,454	Willamette, Clackamas Co., Oregon, U.S.A.	Om	1	14,175:	Fd. 1902	Am. Mus. Nat. Hist.
1051,270	Chupaderos, Jimenez, Chihuahua, Mexico	Of	1	14,114	Kn. for cent.; men. first, 1852	School of Mines, Mexico City
0615,*275*	Otumpa, Tucuman, Gran Chaco, Gualamba, Argentina	D_2S	1	13,636	Kn. 1576; fd. 1783	636kg. Brit. Mus. (Nat. Hist.); 2¼kg. Paris Mus. d'Hist. Nat.; 2kg. Copenhagen Univ.
1057,279	Morito, San Gregoria, Chihuahua, Mexico	Om	1	11,000:	Kn. 1600	School of Mines, Mexico City
1245,428	Port Orford, Rogue River Mountains, Curry Co., Oregon, U.S.A.: poss. mythical	PK	1	10,000:	Fd. 1859	4g. Vienna Naturhist. Mus.
1051,270	Chupaderos, Jimenez, Chihuahua, Mexico	Of	2	*6,767*	Kn. for cent.; men. first, 1852	School of Mines, Mexico City
0382,*130*:*	Bendegó, Bahia, Brazil	Og	1	5,000:	Fd. 1784	Rio de Janeiro
0615,*275*	Otumpa, Tucuman, Gran Chaco, Gualamba, Argentina	D_2S	2	4,210:	Fd. 1923	Buenos Aires (Mus. Nac. Nat. Hist.)
1453,381	Cranbourne, Mornington Co., Victoria, Australia	Og	1	3,500:	Fd. 1854	Brit. Mus. (Nat. Hist.)
1050,*272*:	Adargas, Jimenez, Chihuahua, Mexico	Om	1	3,325	Kn. for cent.	School of Mines, Mexico City
0650,760:	Cape York, Melville Bay, Greenland	Om	2	*2,727*	Kn. 1818	Am. Mus. Nat. Hist.

THE LARGEST KNOWN METEORITES OF THE WORLD (Cont.)

THE LARGEST KNOWN METEORITES OF THE WORLD (CONT.)

C.N.	NAME	CL.	SER. NO.	WT. (KG.)	TM. FD. &c.	DEP.
0486,262	Santa Catharina, Ilha de Sao Francisco, Brazil	D_1	1	2,250:	Fd. 1875	55kg. Mus. d'Hist. Nat., Paris; 45kg. Naturhist. Mus., Vienna
1030,290:	Alpine, Brewster Co., Texas, U.S.A.	D_2S	1	1,820:	Fd. 1915	Mn. ms. *i.s.*; 212g. U.S. Nat. Mus.
1358,224	Huckitta Station, Northern Ter., Australia	P:PI?	1	1,818	Fd. 1936 July 6	South Australia Mus., Adelaide
1078,304	Casas Grandes de Malitzin, Chihuahua, Mexico	Om	1	1,549	Recg. 1867	U.S. Nat. Mus.
1098,365	Navajo, Apache Co., Arizona, U.S.A.	D_2	1	1,503	Fd. 1922	Chgo. Nat. Hist. Mus.
0065,500	Bitburg, Trier, Rhenish Prussia, Germany	PB	1	1,500:	Kn. bef. 1805; fd. 1802	Over 58kg. of smelted material, Berlin Mus.
1453,381	Cranbourne, Mornington Co., Victoria, Australia	Og	2	1,500:	Fd. 1854	Melbourne Mus.
0204,493	Magura, Arva, Czechoslovakia	Og	1	1,500:	Fd. 1840	45kg. Tübingen Univ.; 30kg. Vienna Naturhist. Mus.
0486,262	Santa Catharina, Ilha de Sao Francisco, Brazil	D_1	2	1,500:	Fd. 1875	
1154,381	Quinn Canyon, Nye Co., Nevada, U.S.A.	Om-Og	1	1,450:	Fd. 1908	Chgo. Nat. Hist. Mus.
0982,192	Santa Apolonia, Tlaxcala, Mexico	Om	1	1,316	Fd. 1904	Mn. ms. Inst. Geología, Mexico City; 4kg. Am. Mus. Nat. Hist.; 2½kg. U.S. Nat. Mus.
0248,340:*	Kouga Mountains, Humansdorf Dist., Cape Prov., Union of South Africa	Om	1	1,175	Fd. bef. 1916, poss. *c.* 1903	South African Mus., Cape Town
1205,420	Goose Lake, Modoc Co., California, U.S.A.	Om-Og	1	1,167	Fd. 1938 Oct. 13	U.S. Nat. Mus.
1399,296	Murnpeowie, South Australia	D_3a	1	1,145	Fd. 1909	School of Mines and Industry, Adelaide, South Australia

THE LARGEST KNOWN METEORITES OF THE WORLD (CONT.)

C.N.	NAME	CL.	SER. NO.	WT. (KG.)	TM. FD. &c.	DEP.
0615,*275*	Otumpa, Tucuman, Gran Chaco, Gualamba, Argentina	D_2S	3	1,000:	Fd. 1803	Buenos Aires (Mus. Nac. Nat. Hist.); 636kg. British Mus. (Nat. Hist.)
1025,228	Zacatecas, Zacatecas, Mexico	ObZ	1	1,000:	Fd. bef. 1792	Mn. ms. School of Mines, Mexico City; 5kg. Tübingen Univ.; 3kg. Bonn Univ.
1178,319	Youndegin, Avon, S.W. Div., Western Australia	Og-Ogg	1	929	Fd. 1884	Naturhist. Mus., Vienna
0832,358	Cosby's Creek, Cocke Co., Tennessee, U.S.A.	Og	1	909	Fd. bef. 1837	21½kg. Harvard Univ.; 12½kg. Tübingen Univ.
1453,381	Cranbourne, Mornington Co., Victoria, Australia	Og	3	818	Fd. 1886	Melbourne Mus.
1010,231	Charcas, San Luis Potosí, Mexico	Om	1	783	Kn. bef. 1804	Mus. d'Hist. Nat., Paris
0732,058	Santa Rosa, Tunja, Boyaco, Colombia	D_2S-Ob	1	750:	Fd. 1810	99kg. Chgo. Nat. Hist. Mus.; 5½kg. Tübingen Univ.; 1½kg. Naturhist. Mus., Vienna
0952,321:	Red River, Cross Timbers, Johnson-Smith Co., Texas, U.S.A.	Om	1	743	Fd. 1808	Yale Univ.
0615,*275*	Otumpa, Tucuman, Gran Chaco, Gualamba, Argentina	D_2S	4	732	Fd. 1925	Buenos Aires (Mus. Nac. Nat. Hist.)
0914,560	Krasnojarsk, Yeniseisk, Siberia, U.S.S.R.	PK	1	700:	Fd. 1749	Mus., Leningrad
1041,309	Davis Mountains, Jeff Davis Co., Texas, U.S.A.	Om	1	691	Fd. 1903	Chgo. Nat. Hist. Mus.
1109,322	Tucson, Pima Co., Arizona, U.S.A.	D_2T	1	688	Fd. *c.* 1850	U.S. Nat. Mus.
1098,365	Navajo, Apache Co., Arizona, U.S.A.	D_2	2	685	Fd. 1926	Chgo. Nat. Hist. Mus.
1513,239	Gladstone, Port Curtis, Clinton Co., Queensland, Australia	Ogg	1	659	Fd. 1914	Chgo. Nat. Hist. Mus.

THE LARGEST KNOWN METEORITES OF THE WORLD (CONCL.)

THE LARGEST KNOWN METEORITES OF THE WORLD (CONCL.)

C.N.	NAME	CL.	SER. NO.	WT. (KG.)	TM. FD. &c.	DEP.
0069,436	La Caille, Grasse, Alpes Maritimes, France	Om	1	625	Fd. 1828	Mus. d'Hist. Nat., Paris
1010,237	Descubridora, Catorce, San Luis Potosí, Mexico	Om	1	576	Fd. 1780-83	Mexico City Nat. Mus.; 41kg. Vienna Naturhist. Mus.; 33kg. Chgo. Nat. Hist. Mus.
0996,399	Long Island, Phillips Co., Kansas, U.S.A.	Cia	1	565	Fd. 1891	543kg. Chgo. Nat. Hist. Mus.
0212,293:A*	Ratteldraai, Kenhart Dist., Cape Prov., Union of South Africa	Og	1	550	Fd. 1909	South African Mus., Cape Town
0182,254	Bethany, Great Namaqualand, Southwest Africa	Of	1	503	Fd. 1899	South African Mus., Cape Town
1079,345:	Grant, Zuñi Mountains, Valencia Co., New Mexico, U.S.A.	Of-Om	1	481	Fd. 1929	U.S. Nat. Mus.
0650,760:	Cape York, Melville Bay, Greenland	Om	3	454	Kn. 1818	Am. Mus. Nat. Hist.
1453,381	Cranbourne, Mornington Co., Victoria, Australia	Og	4	454	Unkn.	Melbourne Mus.
1115,297	Carbo, Sonora, Mexico	Om	1	450:	Fd. 1923	Harvard Univ.; 24kg. U.S. Nat. Mus.
0486,*262*	Santa Catharina, Ilha de Sao Francisco, Brazil	D_1	3	450:	Fd. 1875	
0182,254	Bethany, Great Namaqualand, Southwest Africa	Of	2	424	Fd. 1903	
0970,177	Yanhuitlan, Oaxaca, Mexico	Of	1	421	Kn. bef. 1825	Nat. Mus., Mexico City; 17kg. Chgo. Nat. Hist. Mus.
0182,254	Bethany, Great Namaqualand, Southwest Africa	Of	3	404	Fd. bef. 1836	
*0351,*481	Augustinovka, Ekaterinoslav, Ukraine, U.S.S.R.	Of	1	400:	Fd. 1890	Mus., Leningrad

author, the reader is frequently in doubt as to whether the "ton" mentioned is the "short ton" of 2,000 pounds—the common ton of the United States, Canada, South Africa, *etc.*—or the "long ton" of 2,240 pounds—the ordinary ton of Great Britain—or even the "metric ton" of 1,000 kilograms = 2,205 pounds; all that can be done under the circumstances is simply to follow one's own judgment in the matter, which may or may not be correct. It is hardly necessary to add that it would be of inestimable value to meteoritical knowledge if (1) the weights of the largest—as well as of the otherwise most important—meteorites in the world were determined *accurately;* and (2) those weights were expressed in *kilograms* or *grams*—not in "tons" or "pounds" of any denomination!

Two Recent "Falls" in the Portland, Oregon, Area Reported as Meteoritic*

J. Hugh Pruett
University of Oregon, Eugene

ABSTRACT

This article contains an account of two "falls," one that occurred on the night of October 9, 1946, the date of the Draconid meteoric shower, the other on the morning of January 4, 1947, both in the vicinity of Portland, Oregon. These "falls" were thought at the time of their occurrence to be meteoritic in nature, and, as such, were widely reported in the public press, but they were soon proved by scientists and others who investigated them to be unquestionably terrestrial in origin.

Those who deal in matters meteoritical are often expected to determine the nature of a "suspected" specimen from a very meager description. Just recently a 12-year-old boy said to me, "My cousin on the Coast found a rock he thought was a meteor. Was it?" A little more information than this is usually given, yet it often seems quite inadequate! On the afternoon of October 10, 1946, the day after the Draconid meteors staged such a splendid display, a long-distance telephone call came from a newspaper in Portland, Oregon, 125 miles north of my location. The reporter who was calling stated that, the night before, a nurse in Vancouver, Washington, just across the Columbia River from Portland, had seen a blazing meteor fall from the sky and land on the street in front of the car she was driving. The reporter said that the evidence was very strong that this was actually a fragment from the meteoric shower of the night before. My dubious attitude and remark that this was not behavior befitting an orthodox meteorite failed to lessen the reporter's enthusiasm! She ended the conversation with the promise that a piece of the fall would be sent at once for inspection. The next morning, the newspaper gave considerable space on the front page to pictures of the fragments and of the finder as she held one of these, and to a description of the phenomenon. The second paragraph of the account read:

> "A fragment which stumped analysis of Dr. John Allen of the State Department of Geology and Mineral Industries [at Portland] has been sent to Dr. J. Hugh Pruett, Eugene astronomer, for inspection."

The story of the fall as related by the nurse, Mrs. Marshall Cowie of Vancouver,

*Read at the 10th Meeting of the Society, 1947 June.

Washington, was about as follows: Mrs. Cowie was taking her mother to the railroad station at 11:10 P.M., when suddenly a ball of fire appeared thru the windshield of her car. It fell straight downward at the side of the street, landed only 50 feet ahead of the car, and almost immediately went out. The nurse turned her car so that the headlights would strike the place of fall. A bystander reported that the car's lights illuminated a column of white smoke that arose from the spot and was visible for about 4 feet upward from the ground. Mrs. Robert Kadow, living near the place of fall, said that the dining room of her home was illuminated by the light. She, her husband, and her brother ran outside and saw the "white-hot ember" strike the surface. They picked up a few "globules" of the material from the spot, which they reported had seemed to be falling "straight down." The nurse said that the fallen fragments were so hot that they burned her fingers when she finally found them by feeling around in the grass and gravel. She got some of them into her purse and drove on to her mother's home, where, 10 minutes later, they were "still too hot to hold." The newspaper account seemed to indicate that the thing might have fallen in one piece, egg-shaped and about 2 inches long; it looked somewhat like burned bone, and it crumbled easily. The account ended thus: "An inspection of the spot in daylight Thursday [the next day] revealed that the gray pebbles were scorched and browned for about 4 to 6 inches on either side. A small scraggly weed growing there had several burned branches on the side nearest the fragment."

On the afternoon of the day the account appeared in the newspaper, the specimen arrived, securely wrapped and well insured. Out of about 1,000 "suspects" received by me in the course of the past 15 years from western Canada and the United States, only 3 have been meteorites! One of these was the Washougal, Washington, howardite, and the other two were "misplaced" Canyon Diablo, Arizona, specimens found loose in Oregon and Montana. Accordingly, there was little hope regarding the "Vancouver object," and especially so because of the story connected with its fall. The first glimpse of it did not increase the hope. It was principally white, slightly glazed on one side, and had the appearance of an object that could be easily crumbled by the fingers. Stuck into the white mass were many dark bits of what appeared to be roadside gravel. The white part looked almost exactly like a sample received from a farmer near Portland a year earlier, who said that not any of it was near the gateway of his hop yard one afternoon, but that next morning a string of fragments stretched along 100 yards outward into the field. Mahlon Sweet, an aviation authority at Eugene, thought that both this and the Vancouver object were remnants of magnesium flares dropped by airplanes. Rough chemical tests for nickel and iron gave no positive evidence of the presence of either. In the hope that the specimen might contain magnesium compounds, the sample was given to Professor Roy C. Andrews of the Department of Chemistry of the University of Oregon for a test for this metal, but he reported that he found no magnesium. Then Dr. Lloyd Staples of the Department of Geology spent several days making crystal and other tests. Dr. Staples reported that "these fragments are high in potassium and sulfur content, but they contain no magnesium or any of the other elements most characteristic of meteorites." The specimen was finally sent to Dr. Frederick C. Leonard, the meteoritical expert at the University of California, Los Angeles. He soon reported, "Your specimen is not a meteorite; it is a *meteorong!*" Even should we grant a small meteorite the curious behavior of glowing all the

way down to the ground, it seems unusual that, with the clear skies that were prevailing that evening over all of western Oregon and Washington, no report of a brilliant meteor should have been received from any place in that region except the town of Vancouver. The object's connection with the Draconid meteoric shower seems ruled out on another count; of the 20 spectra of those meteors obtained by Dr. Peter M. Millman of the Dominion Observatory of Ottawa, Canada, he reported that all were alike and indicated the presence of sodium, iron, magnesium, manganese, and possibly calcium.

Around noon on January 4, 1947, there came another long-distance call from the Portland newspaper. A "meteoritical" opinion was wanted on a mysterious "fall" at Hill Military Academy at 9:45 o'clock that morning. While Captain Leon G. Thompson, Commandant of Cadets at the school, was inspecting the grounds, suddenly there descended from the skies and fell near him a curious shower of ice pellets and a single concrete-appearing, ice-covered object that had somewhat the shape of a truncated cone. The Captain reported that the shower lasted 2 or 3 seconds and covered only a few square feet of surface. He inspected the skies for a chance airplane, but could see none. This story spread like wildfire as soon as the press got it. Almost any radio station one might tune in was soon reporting it. The newspapers all over the country were discussing the strange phenomenon. Dr. John Allen, the State Geologist at Portland, mentioned earlier, was asked to make a complete analysis of the material. His chemist did a careful piece of work on it, and reported, both qualitatively and quantitatively, on the composition of the strange object. On January 7, the newspaper gave a double column, the full length of p. 1, to the copyrighted article discussing the analysis and pointing out the startling discovery that the object might possibly have been part of a rocket made in this country or elsewhere. The refractory material revealed by the analysis was said to bear out this contention. Indications seemed strong that the thing might have come from White Sands, New Mexico, or even from Russia! Two more calls came from Portland that day. A newspaper reporter wanted me to determine from the published analysis whether or not the object was a meteorite. When I would not commit myself, she promised to "get you a sample of the darned thing, if it is humanly possible." (It was evidently *not* "humanly possible"!) The other call came from the professor of astronomy at one of the Portland colleges. From the published descriptions, he was sure the "Hill Military Academy object" was a meteorite, and he wanted my aid in preventing its being taken away from Portland, as the Army was planning to do. Later reports indicated that it had been taken to San Francisco, California, and finally to Washington, D. C. This was on Tuesday, 3 days after the "object" had "tumbled from the sky." At noon the radio newscasts were still excitedly extolling the wonderful object and its possible source; but, by 3 P.M., the radios had a sudden change of story: the "meteorite" or "rocket" had been completely deflated! A curious newspaper reporter, snooping around the Academy grounds, had discovered that the stucco plug was missing from one of the old artillery shells that ornament the campus; only a few fragments remained in it. An analysis of this plug gave exactly the same results as did the analysis of the cone-shaped object! (It was freezing weather at the time.) The newspaper later received an anonymous telephone call from a supposed student at the school, which indicated that the whole affair was a prank against the military instructor! Captain Thomp-

son later wrote me that, if he could ever again get his hands on the mysterious object, he would surely send me a sample of it; but no sample has yet arrived from any source.

President of the Society: ARTHUR S. KING, Mount Wilson Observatory, Pasadena 4, California
Secretary of the Society: OSCAR E. MONNIG, 1010 Morningside Drive, Fort Worth 3, Texas

VARIABLE STARS

Variable Star Notes from the American Association of Variable Star Observers

By LEON CAMPBELL, Recorder

Annual Meeting of the A.A.V.S.O.: The 36th Annual Meeting of the Association was held at Harvard on October 10 and 11, ending with its participation in the organization meeting of the Northeastern Regional meeting of the Astronomers League on the 12th. Mild weather with cloudless skies prevailed throughout the entire weekend, and members were in attendance from Maine to Wisconsin. The Council met on Friday afternoon in the lounge, first to partake of tea, sandwiches, and cookies, then to attend to details of administration as well as election of new members.

Twenty-two candidates were admitted to active membership, including one each from Belgium, China, Greece, and Uruguay. Two of our outstanding observers of long years of experience were elected to honorary membership; The Reverend T. C. H. Bouton of St. Petersburg, Florida, a veteran of 35 years in the Association, who celebrated his 90th birthday anniversary in November, 1946, and Mr. R. G. Chandra of Bagchar, India, an observer since 1920. Both may now be considered in the "emeritus" ranks.

On Friday evening the Association was host to the Bond Astronomical Club, with Dr. Smiley as speaker. He spoke on his 1947 trip to Brazil and return, entitling the illustrated talk, "15,000 Miles and 15,000 Observations." With the splendid kodachrome slides, and the speaker's inimitable style of lecturing, the audience kept ever alert and there was not a dull moment throughout the discourse. With clear skies prevailing, some of the younger members took advantage of the opportunity to observe with the 6-inch Post telescope. Mira, now at its 1947 maximum, also came in for its share of naked-eye attention.

The regular business session was held on Saturday morning, with about fifty persons present. Reports of officers and committees were received, eulogies were paid to those members who had passed away during the year. These included Dr. William L. Holt, who, it will be recalled, contributed the millionth observation in 1946; Dr. H. H. Clayton, a world-famed meteorologist of some sixty years' standing; Mr. Knox Lee of Marshall, Texas, an enthusiastic member of the Solar Division; Mr. William B. Stearns of Milton, Mass., for many years a promoter of astronomical research in secondary schools, especially at Milton Academy; and Mr. Julius F. Stone of Columbus, Ohio, one of the AAVSO patrons and a generous benefactor to science in general.

Newly elected council members are R. N. Buckstaff, Oshkosh, Wisconsin, C. F. Fernald, Wilton, Maine, C. B. Ford, Windsor Locks, Connecticut, P. M. Millman, Dominion Observatory, Ottawa, Canada. Officers for the coming year are: President, Dr. Marjorie Williams, Smith College, Northampton, Mass., First Vice-President, former secretary D. W. Rosebrugh of Waterbury, Connecticut, Second Vice-President, Solar Division Chairman, N. J. Heines of Paterson, New Jersey. The offices of secretary, treasurer, and recorder are held by the incumbents of recent years, Messrs. Harris, Witherell, and Campbell, respectively. Retiring president Smiley goes on the Council for four years, and past president Seely remains on the Council for two years more.

The recorder's 16th Annual Report indicated a marked increase in newly acquired observers, as well as greatly increased contributions of observations. Reports were received from 39 observers in 11 foreign countries, including 11 in Greece, 5 each in Australia and Canada, 3 each in South Africa and India, 2 each in Germany, Argentina, and Mexico, and one each in Italy, Egypt, Belgium, Denmark, Peru, and Czechoslovakia.

The AAVSO Library showed an increase of 266 acquisitions. Some 40 duplicates of AAVSO volumes were donated to an observatory in Czechoslovakia. A 3-inch telescope was loaned to a student at Rensselaer Polytechnic Institute for summer use, and a 5-inch refractor was loaned to Professor Menzel for testing the seeing conditions in the Southwest.

During the year the Association issued its Bi-monthly Bulletins, three numbers of its own Variable Comments, the 1946 reprint of Variable Star Notes, addenda to the 1946 membership list, and a new edition of the Revised Constitution and By-Laws. Harvard Annals 110, Addenda, and 116, No. 1, were also issued, the latter containing some twenty-two thousand original observations of long-period and special variables. The grand total of observations for the year was slightly over 50,000, an increase of nearly 6,500 over last year, and almost rivalling the years just before the recent war. One hundred and fifty-five persons took part in the observing programs, 95 of whom can be considered as fairly regular contributors. The foreign observers contributed 22,427 estimates, or 44% of the whole, while the American total was 28,000. Cyrus F. Fernald of Wilton, Maine, headed the list with 6649 to his credit, and R. P. de Kock of Capetown, South Africa, was next with 4616 observations. C. Chassapis of Athens, Greece, followed with 3502. Paul Ahnert of Sonneberg, Germany, contributed 3391, and L. C. Peltier of Delphos, Ohio, was fifth, with 2431 observations.

The special SS Cygni and R Coronae Borealis-type variables came in for very close attention during the year. Three of the four R Coronae Borealis stars were at minimum at one time or another, SU Tauri being the only one which did not drop to a minimum. The six maxima of SS Cygni were very completely observed; that of September being the 360th observed since the discovery of the star in 1896; and not one maximum has been missed. The bright naked-eye variables in Cassiopeia received considerable attention, especially by observers who were obliged to observe without optical aid.

The nova search work moved slowly but surely, with the hope that should a bright nova appear, it will be detected without delay.

A voluminous report of the Solar Division was presented by its very active chairman, Mr. Neal J. Heines. A membership of 94 is represented among ob-

servers distributed over 23 states and in ten foreign countries, 47 of whom are sending in monthly reports. Nearly 5000 observations were reported for the year. All observations were reduced to a Preliminary Relative Sunspot Number at the Bureau of Standards, under the direction of Dr. A. G. McNish; the actual reductions being made by Mr. Alan H. Shapley and Miss J. Virginia Lincoln. Five special bulletins of the Division were issued.

From the solar and ionospheric standpoint there are indications that the present maximum in solar activity has been reached. More phenomenal spot groups are still to be expected, accompanied by magnetic storms and radio fade-outs.

The report of the Occultation Committee, by Dr. Alice H. Farnsworth, showed continued activity and increased results. A total of 120 observations of occultations were received during the year 1946, and all but six have already been sent to England for reduction; this is 50 more than in 1945. These 120 observations represent the work of 31 observers. E. A. Sill of Mamaroneck, New York, contributed 25, O. E. Monnig of Fort Worth, Texas, 13, Donald Mary of Louisiana State University, 11. To date in 1947 there have been 58 observations communicated. A plea is made for more observers, especially those equipped with suitable telescopes and accurate time facilities.

Following the conclusion of the business meeting, a series of 12 papers was presented.

1. Novae in the Sagittarius Field, Mrs. M. L. Mayall
2. Science or Modern Astrology, Herbert Luft
3. Variations of Alpha Cassiopeiae, H. R. Petzold
4. Spherical Projections of Sunspots, W. L. Moore
5. The Hydrological Cycle (Title only), F. J. Ryder
6. Note on Foreshortening of Sunspots and Decrease of their Numbers Towards the Limb, W. H. Gleissberg
7. Granulations on Sun's Surface (Title Only), J. C. Bartlett, Jr.
8. The Yalden Reflex Camera, N. J. Heines
9. Changing Periods of Long-Period Variables, L. Campbell
10. Testing a Refractor, D. W. Rosebrugh
11. Period of Eta Geminorum, G. Matthews
12. Remarks on H. L. Alden's Note on the Rapid Variations in AE Aquarii, L. Campbell

The annual dinner was served in the lecture hall, deliciously prepared and ably served by the ladies of the observatory staff. Following the meal Mr. Rosebrugh spoke on the "Status of Lunar and Planetary Observing in this Country." Mr. Buckstaff reported on the progress made with the erection and adjustment of his new 16-inch reflector. Dr. Shapley climaxed the occasion with the recounting of the 1947 Highlights in Astronomy. Among these were mentioned:

1. The great sunspot group of April, some 50 times the area of the earth.
2. Photographs of the Sun's spectrum extending well into the infra-red, accomplished by shooting rockets above the ozone layer of the Earth's atmosphere.

3. The ages of meteorites as studied by Carl A. Bauer, in which he finds a correlation between size and helium content; the smaller the meteorite, the older, and the richer in helium.
4. The completion of the 200-inch mirror.
5. The gift of Harvard's 8 and 11-inch telescope to war-torn observatories in Poland and China, respectively.
6. The work of Babcock and Chapman on measures of magnetic fields in the stars.

The evening closed with a very effective demonstration of the new Spitz planetarium, in which projection is accomplished without the use of projection lenses.

Observations received during September: Observations contributed during September by 74 observers totaled 7393—a record month for many years.

Observer	No. Ests.	No. Var.	Observer	No. Ests.	No. Var.
Adamopoulos	15	15	Krumm	9	14
Ahnert, P.	33	430	Lee	11	11
Albrecht	16	23	Luft	4	24
Ancarani	23	60	Lundquist	3	3
Ball	17	30	Marrison	2	4
Bicknell	15	92	Matthews	16	57
Blunck	31	35	Miller	23	28
Bogard	26	33	Nadeau	111	214
Boone	15	15	Oravec	41	127
Buckstaff	19	73	Paletsakis	16	880
Canfield	5	15	Papadopulos	4	22
Caraioryis	10	14	Park	3	13
Chandra	24	38	Parks	28	28
Chassapis	132	900	Pearcy	55	70
Cilley	26	50	Peltier	167	276
Cragg	67	70	Penhallow	3	27
Daley	5	11	Petzold	67	584
Elias	80	257	Plybon	5	11
Escalante	29	31	Reeves	2	10
Estremadoyro	3	6	Renner	103	103
Ewen	2	3	Rosebrugh	12	125
Fernald	273	920	Sandage	20	28
Focas	56	108	Schoenke	14	20
Ford	21	24	Segers	27	100
Garneau	23	23	Sherman	13	17
Gingerich	2	2	Sill	11	11
Greenley	28	69	Thomas	32	40
Guimont	2	2	Tifft	12	43
Halbach	11	11	Upjohn	18	24
Harris	24	24	Wade	7	7
Hartmann	113	118	Webb	26	26
Howarth	13	13	Weber	30	30
Karle	3	3	Welker	43	72
Katsis	18	161	Wells	3	5
Kearons	39	49	Whittier	3	3
Kelly	12	16	Zirin	22	38
Kitley	49	102			
de Kock	108	426	74	Totals	7393

October 15, 1947.

Comet Notes

By G. VAN BIESBROECK

Two new comets have been added in September to the already long list under observation.

COMET 1947 *j* (REINMUTH). The first was found photographically on September 10 at Heidelberg (Germany) by J. Reinmuth. Data of discovery:

1947 September 10.9125 Universal Time
Right ascension $23^h\ 26^m9$, Declination $+9°\ 52'$. Magnitude 13.

The motion was given as slow in a northwestern direction. This information, relayed over the Central Bureau in Copenhagen, did not reach this country until September 15, but the object was sufficiently well followed for enabling L. E. Cunningham to have an orbit computed by September 22. This showed that it was a short-period comet moving near the plane of the ecliptic. Already in 1928 Reinmuth discovered a periodic comet which was observed at its second apparition in 1935, but missed at its 1942-43 return.

The present comet will now be designated as PERIODIC COMET REINMUTH 2. The following elements by Cunningham have been improved over the preliminary computations:

Perihelion passage	1947 Aug. 24.30297	Universal Time
Node to perihelion	46°.79891	
Longitude of node	296°.62474	1947.0
Inclination	7°.01959	
Semi-major axis	3.446013 a.u. (period 6.397 years)	
Eccentricity	0.4603877	

The brightness is decreasing so that the comet can only be reached by large instruments. Being well situated in Pisces, it will probably be followed until the end of the year. On October 9 it was recorded here as a nearly round coma some 20 inches in diameter, slightly condensed centrally. The magnitude had dropped to 14.

COMET 1947 *k* (BESTER). Not two weeks had gone by when one more new comet was announced. This one was found by Bester in South Africa: it is the fourth one discovered recently by this very active observer. The information was:

1947 Sept. 25.08889 U.T.
Right ascension $4^h\ 47^m$, Declination $-19°\ 35'$. Magnitude 11.

From the data available by October 3, L. E. Cunningham and A. D. Maxwell independently deduced very similar parabolic orbits:

	L. E. Cunningham	A. D. Maxwell
Perihelion Time	1948 Feb. 17.727	Feb. 17.195 U.T.
Node to perihelion	348° 50′	349° 24′
Longitude of node	270 02	270 17
Inclination	140 12	140 12
Perihelion distance	0.7677	0.7596

The ephemeris shows rapid southward motion so that by the end of October the comet will be in reach only in the southern hemisphere. However, next February the motion is northward and by March the brightness will increase so much that the naked-eye visibility may be reached.

As recorded here (Oct. 9) the comet showed a well-defined nucleus and a short tail in 340°. The magnitude was estimated as 10.

The many faint comets mentioned last month are still in reach of larger telescopes; they are all fading in brightness except periodic comet Encke. Last month's ephemeris shows that this well-known comet should be easily picked up with small telescopes in November. Estimates of brightness of this morning object are very desirable.

Williams Bay, Wis., Oct. 13, 1947.

General Notes

Dr. Daniel M. Popper, formerly of the Yerkes and McDonald Observatories, was, on July 1, 1947, appointed Assistant Professor of Astronomy in the University of California, Los Angeles.

Professor E. Delporte, director of the Royal Observatory of Belgium, was made honorary director as of January 31, 1947, and now resides at 151 Groeselenberg, Uccle. Professor Paul Bourgeois, astronomer, has been promoted to the position of director of the Observatory.

Grote Reber, 35-year-old radio physicist and engineer, has been appointed to the staff of the National Bureau of Standards, according to an announcement by Dr. E. U. Condon, Director of the Bureau. Mr. Reber, who has conducted basic research on the sources of radio noise, will direct several new projects at the Bureau aimed at extending present knowledge of conditions indirectly affecting radio communication. His investigations will center on the study of cosmic and solar radio noise—thought to be electromagnetic radiation, like heat or light, originating in outer space and in the sun. He is supervising the erection of a German Giant Wurzburg, an extremely large and powerful radar device, that will be used to detect the solar and cosmic radiation which penetrates the earth's atmosphere.

American Astronomical Society Meeting

The seventy-eighth meeting of the American Astronomical Society, according to an announcement by the Secretary, Dr. C. M. Huffer, will be held at the Emerson McMillan Observatory of the Ohio State University, Columbus, December 28-31, 1947. The members and guests will be housed in Neil Hall on the campus of the University. On December 30 two symposium sessions and luncheon will be held at the Perkins Observatory, near Delaware, Ohio.

The International Astronomical Union, according to information recently received from the General Secretary, Dr. J. H. Oort, will hold its next General Assembly in Zürich, Switzerland, from August 11 to August 18, 1948. The general meetings of this international organization were of necessity suspended during the war. The coming General Assembly will be held almost exactly ten years after the preceding one which was held in Stockholm, Sweden, in 1938, August 3-10.

The Rittenhouse Astronomical Society of Philadelphia held its monthly meeting on Ocotber 10, 1947, in Randal Morgan Physics Laboratory, University of Pennsylvania. Mr. John H. Pitman, Sproul Observatory, Swarthmore College, spoke on the "Magnitudes of Cepheid Variables."

Astronomers Needed

The Civil Service Commission has announced an Astronomer examination for filling positions in Washington, D. C., and nearby Virginia and Maryland. The salaries range from $3,397 to $7,102 a year.

To qualify, applicants must (a) have completed a full 4-year college course leading to a bachelor's degree in astronomy, or (b) have had 4 years of successful and progressive technical experience in the field of astronomy, or (c) have had any time equivalent combination of (a) and (b). In addition to meeting these prerequisite requirements, applicants must have had from 1 to 4 years of progressive professional experience in astronomy. Graduate study in astronomy may be substituted for as much as 2 years of professional experience.

No written test is required. The age limits, eighteen to sixty-two years, are waived for persons entitled to veteran preference.

Full information and application forms may be secured at most first- and second-class post offices, from civil service regional offices, or direct from the U. S. Civil Service Commission, Washington 25, D. C. Applications will be accepted in the Commission's Washington office until further notice.

The Atmosphere of Mars

It is well known that the question whether or not life exists on other worlds revolves around the problem whether life exists on Mars. The planet Venus has an atmosphere largely composed of carbon dioxide and is devoid of oxygen and water. It seems unsuitable for life. The giant planets with their extended atmospheres of methane, ammonia, and possibly hydrogen, all under temperatures of two hundred or more degrees F below zero are certainly incapable of supporting life. Mars, however, shows polar caps like our globe, shows occasional clouds and green areas, changing with the seasons, as well as extended deserts which occasionally develop dust storms. Here at last we seem to find a world like ours.

But the spectroscopic tests of its atmosphere to date have been disappointing. At the Mount Wilson Observatory it was found years ago that very little if any water vapor was present in the Mars atmosphere and also very little if any oxygen. What then is the atmosphere composed of?

During the last week spectra of Mars have been obtained by Dr. Gerard P. Kuiper, Director of the Yerkes and McDonald Observatories, which show carbon dioxide to be present on Mars, not in the huge quantities it is found on Venus but in the very modest quantity found on Earth. Only 1/3000 of our own atmosphere is carbon dioxide. The Mars results were obtained with the new infrared spectrometer which was developed last year, partly in collaboration with Professor Cashman of Northwestern University. The spectrometer contains a tiny cell, the size of a dime, which is sensitive to the heat rays of the infrared spectrum.

The two new carbon dioxide bands found in Mars have wave lengths three times as long as visible light.

Dr. Kuiper further reported that the poisonous gases such as ammonia and methane, so abundant on the giant planets, are absent on Mars, so that life is indeed possible. The bulk of the atmosphere might be made up of nitrogen which does not have bands in the observable part of the spectrum.

In view of the small amount of carbon dioxide found on Mars we may conclude that the polar caps are probably not dry ice but ordinary snow and clouds, made of water. When Mars gets to its closest approach to the Earth on February 18, 1948, the polar cap may be big enough in the telescope to allow its spectral observation. If so, a decision will at least be possible as to the amount of water present; all we know now is that it is small. Also the green areas may then be effectively analyzed. In this manner the use of infrared rays promises to solve the age old problem—is there life on other worlds?

Sir Isaac Newton's Correspondence

Although the tercentenary of the birth of Sir Isaac Newton occurred in 1942, the commemoration of it was deferred until July, 1946. This celebration was held in London and Cambridge under the auspices of the Royal Society. It was attended by 148 delegates from almost all of Europe, Asia, South Africa, USSR, South America, and the United States, among them the noted physicist, Max Planck, then 88 years old.

An outgrowth of this event is the decision on the part of the Royal Society of London to collect and publish all of the voluminous correspondence of Sir Isaac Newton and the replies thereto that can possibly be found. Much has already been published, but much more is known to have been scattered widely. Accordingly the Royal Society has appointed a committee under the chairmanship of E. N. da C. Andrade, professor of physics in the University of London, to carry out this plan. Mr. Frederick E. Brasch, Honorary Consultant in the History of Science, Library of Congress, Washington, D. C., is a member of the committee.

Any persons in this country who, perchance, may have material or information pertinent to this undertaking are requested to communicate with him and thereby contribute to the success of a project which will stand as a monument to the genius and versatility of the man who has brought such renown to the English speaking peoples.

New Statistical Method of Predicting Sunspots

The prediction of solar activity, which greatly affects radio communication and is evidenced by spots on the sun, has been advanced through the application of a new statistical method, by A. G. McNish and Virginia Lincoln of the National Bureau of Standards. The new technique, depending on available sunspot data for a number of previous 11-year cycles, has a sounder scientific basis than former methods of prediction. Moreover, it is expected to be applicable to a wide variety of cyclical phenomena, such as long-term weather variations and climatic changes.

Accurate sunspot prediction became necessary at the Bureau in connection with its radio prediction service, which each month issues radio propagation

predictions three months in advance. This information enables radio engineers to calculate the best usable frequencies for communication between any two points in the world for any hour of the day.

The highest monthly average sunspot number in over 100 years, and one of the highest of all time, occurred during May, 1947. This abnormal burst of solar energy, accompanied by a series of ionspheric disturbances of all kinds throughout the world, may represent the peak of the current sunspot cycle, which is proving to be one of the highest. The high value in May was foreshadowed by the appearance of the three largest sunspot groups ever observed—in February, 1946, in July, 1946, and in March and April, 1947. The group observed in February, 1946, covered an area on the sun's surface more than 100 times the area of the earth's disc. At the lowest point of a cycle, a monthly sunspot number of zero or almost zero is not unusual. As recently as April, 1944, only one sunspot, appearing for a single day, was observed for the entire month.

(From Technical Report 1104, National Bureau of Standards)

On the Observation of Meteors in a Zeiss Planetarium

In the Zeiss-Planetarium in The Hague meteors can be projected with an instrument composed of 3 rotating disks, with slits thru which the light falls on the dome. The first disk has a number of slits diverging from the center of the disk, the second, which rotates in the other direction has a few slits forming a spiral. The third rotating disk consists of a few sectors. The object of the observations in the planetarium is to determine the errors of the observations of observers with different training in the field of meteor observing. However, it must be remembered that the errors in the observing of planetarium meteors are about two times as large as with real meteors because the phenomena of the planetarium meteors is not the same as that of real meteors, and because the observer sees the meteor under a smaller angle in the planetarium "sky" than in a real sky as he does not observe from the center of the dome.

The errors of observation may be divided into: *(a)* error of beginning and ending point; *(b)* "twist" of the path; *(c)* "shift" of the path; *(d)* distance from radiation point to flight direction.

In reducing the observations the observers may be divided into three main groups. In our case we had one or two observers in each group.

A. The observer has observed 0-100 meteors; B. The observer has observed 100-1000 meteors; C. The observer has observed over 1000 meteors.

During the observations about 10 meteors per hour were projected on the dome to imitate real observing conditions. After each observation the meteor was projected again very slowly and its real path determined. In this way the errors of observation could be determined afterwards. About half of the meteors were observed without aids, the rest by using a string (Prentice method).

The difference between the errors of beginning and ending point is very much smaller than is usually thought. One of the most interesting results was that the mean path length was 15 % too short! The maximum error was 24% too short and 36% too long. The errors are the greatest for short paths. The greatest part of the unobserved portion of the luminous path lies before the point where the meteor was first seen.

I hope that these observations will not only prove of help to computers of meteor heights and radiant points but will also give greater confidence in visual meteor observations.

Wassenaar, Holland, September 1, 1947. SIDNEY VAN DEN BERGH,
President, Meteor Section, Neth. Astron. Soc.

Book Reviews

Annals of the New York Academy of Sciences, Volume XLVIII, Article 8, pages 705-844, has recently been issued. This part consists of a paper entitled "Convection Patterns in the Atmosphere and Ocean" under the authorship of 12 men whose names are well known in this field. The price stated is $2.00. It may be secured from the Executive Secretary, The New York Academy of Sciences, Central Park West at 79th St., New York 24, N. Y.

Observatory of Padua Observations,—Four pamphlets from this Observatory have recently been received, bearing the dates of issue 1940, 1941, 1945, and 1946, respectively. They contain a collection of papers bearing the numbers from 63 to 86, inclusive. The topics treated are very varied, all, however, such as are properly considered by an active and an alert observatory staff. Naturally they are in the Italian language. Apparently the activity of the Observatory was lessened during the war years. The surprise is that any work at all of this nature was accomplished.

The first three are entitled as emanating from the R. Osservatorio Astronomico de Padova, whereas the last one significantly is from the Osservatorio Astronomico de Padova.

Astronomy, by Freeman D. Miller. (Bellman Publishing Company, Inc., Boston, Massachusetts. Seventy-five cents.)

This is a pamphlet of 32 pages. It is No. 72 of a series of Vocational and Professional Monographs.

This pamphlet touches upon the usual topics one thinks of when the subject of astronomy is mentioned. It also treats briefly such questions as, The Organization and Work of a large Observatory, Why Astronomers like their Profession, Personal Qualifications desirable in an Astronomer, Employment Opportunities for the Professional Astronomer, and others. It also discusses the advantages and disadvantages of an astronomical career, astronomical organizations, and astronomical publications. All in all it will be found to be a very useful handbook for a beginner in the science of astronomy.

Sun, Moon and Stars, by W. T. Skilling and R. S. Richardson. (Whittelsey House, McGraw-Hill Book Co., Inc., New York $2.50.)

This volume of 268 pages and index came from the press in 1946 but has not been mentioned before this in these pages. The authors, both men of experience in astronomical teaching and observation, have touched upon the salient topics in a way to bring the simpler facts of astronomy clearly and easily within the reach of the beginner. The book is divided into five parts, namely, The Moon,

The Sun, The Sun's Family of Planets, The Stars, and Astronomers and Observatories.

The format is attractive, the style easy, and the photographs and drawings numerous. All in all it may be highly recommended to one wishing to begin the study of this extensive and growing science. C.H.G.

Apparent Places of Fundamental Stars, 1947. (His Majesty's Stationery Office, London, England. £2 2*s* 0*d*.)

This large-sized volume of 538 pages contains as its main feature the apparent places, for every tenth upper transit over the Greenwich meridian, of 1483 stars whose declinations lie between ±81°. This is the seventh issue of a similar volume whose publication was begun in 1941. It is produced by international co-operation under the auspices of the International Astronomical Union. Normally it is to appear in May of the year preceding that to which it refers.

It is believed that this volume will be found indispensable by all observatories engaged in fundamental observations, particularly those for the determination of time; it will also be of material service to institutions engaged on accurate surveys, for which the selection of stars in the Almanac may not be adequate.

In the United States of America this volume may be obtained from British Information Services, 30 Rockefeller Plaza, New York 20, N. Y.

Planetary Co-ordinates for the years 1940-1960, prepared by H. M. Nautical Almanac Office.

The original edition of this volume (published in September, 1939) has been out of print since 1940 when the type, plates, and stock were destroyed. A photographic reprint has now been published, and can be obtained, at a price of 17s. 6d., from H. M. Stationery Office, through their agents and through any bookseller.

The volume contains the heliocentric spherical and rectangular co-ordinates, referred to the equinox of 1950.0, of the seven planets from Venus to Neptune, together with their attractions on the Sun, from 1940 to 1960. The interval is 10 days for Venus, Earth, Mars, Jupiter, and Saturn and 40 days for Uranus and Neptune. Spherical co-ordinates (only) are also given for Uranus and Neptune at intervals of 200 days. Many auxiliary tables required in connection with the calculation of planetary perturbations and cometary orbits are included and a detailed illustration of use of the tables for this purpose is given. Published by H. M. Stationery Office, York House, Kingsway, London, W.C.2.

Mathematical Tables, Eighth Edition, compiled by Charles D. Hodgman. (Chemical Rubber Publishing Co., Cleveland, Ohio, 1946.)

Since its first edition as a rather thin little volume in 1931 "Mathematical Tables" has grown thicker and more complete in proportion to "Handbook of Chemistry and Physics" from which it is substantially reprinted. The "Handbook" has become almost too big for one hand, but the "Tables," although printed on heavy paper, remains still a small reference work. It is at the same time the best available one volume general reference book on mathematics. Besides rather com-

plete tables of elementary functions, interest, etc., there are collections of formulae from college mathematics and almost four hundred integrals.

There is little to suggest by way of improvement of the tables. The publishers note that no errors have been discovered in the major tables since the last printing. Perhaps some refurbishing would be welcome in parts of the text which have remained unchanged for years. For example, "Since most numbers are incommensurable powers of ten . . ." (p. 1) is neither precise nor in accord with current usage. "*Almost all* numbers are *irrational* powers of ten" would be preferable. A more clear-cut rule for finding the characteristic of a logarithm might well replace the present method (p. 2) which involves rewriting the number. These are minor matters.

Recent additions to the "Tables" are all welcome. Among these, "Abbreviations and Symbols of Mathematics" is a good beginning of what might be a very useful section, but it needs considerable revision. It includes well laid out parts on Commercial Arithmetic, Financial and Actuarial Theory, and Plane Geometry. The remainder of the section is a more or less alphabetical collection, which has the appearance of notes rather than a finished reference work. Columnar arrangement would help. To make the section useful, however, the main thing is to continue compartmentalization of the material along the lines already begun in the sections mentioned above. Abbreviations of words should be separately alphabetized. Latin and Greek letters, not used as abbreviations, should be ordered in clearly separated sections. Non-alphabetical symbols might be arranged according to subject or function—the latter being a less vague type of classification (*e.g.* symbols of aggregation, symbols of relation, symbols of operation, logical symbols, etc.). Finally, this part of the book might be weeded of information belonging in the existing reference sections on Calculus, Algebra, etc., and of side comments of a controversial nature. As an example of the latter, we mention the statement that right-hand coordinate systems in space are "recommended." By whom, in view of the general use of left-hand systems? On the other hand, the admonition "Do not use the obsolescent form $\angle n$ for $n!$" is justified by almost universal usage.

"Mathematical Tables" is already such a good reference work that one is tempted to urge that it be enlarged and improved still more. Completeness is impossible of course but there is still room for expansion without making the volume unwieldy. Enlargement of the table of integrals would make the owner independent of the brief tables of integrals now in general use. Inclusion of tables of the Chi-square, the Student's t, and the F distributions would be justified by the wide use to which these are put in testing statistical hypotheses. Addition of further reference sections including more advanced topics would also be welcome. These and other accretions could make the "Tables" a more useful, even indispensable reference work for every student and user of mathematics. The additional material might not find a place in the "Handbook of Chemistry and Physics," but "Mathematical Tables" is now sixteen years old and perhaps ready for a separate existence.

KENNETH MAY.

Carleton College.